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EXTINCTION FOLLOWING PARTIAL AND VARIED REINFORCEMENT

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Yale University[1]

A partial reinforcement training procedure typically results in greater resistance to extinction than continuous reinforcement (**6**). A number of hypotheses have been advanced in an effort to account for this empirical generalization (e.g., **2, 5, 9, 10, 13, 14**). None of these appears completely satisfactory within an S-R reinforcement frame of reference, although some may provide a partial answer. It is our intent to provide new data which bear uniquely on this problem, and to offer one analysis which these data suggest.

Partial reinforcement confounds the effects of two more basic operations: varied delay of reinforcement and varied magnitude of reinforcement.[2] That is to say, magnitude of reinforcement is sometimes a finite quantity and sometimes is zero; likewise, delay of reinforcement is sometimes a finite interval and sometimes is (effectively) infinite. It was our belief that a study of varied delay and varied magnitude separately would place partial reinforcement in a more complete systematic perspective.

Accordingly, during training for one group of Ss the delay of reinforcement was varied randomly between two equally likely values while magnitude was held constant; for another group only magnitude of reinforcement was varied. For each of these experimental groups there were two control groups, one of which received a constant reward at the preferred of the two values and the other at the mean of the two values for the experimental groups. The acquisition results were presented elsewhere (**8**); briefly, the varied delay group attained a performance level comparable to that of its preferred delay control group whereas the varied magnitude group closely approximated its mean magnitude control group. This paper presents the relative rates of extinction following these training conditions, together with other relevant data.

[1] This research was supported in part by a grant from the National Science Foundation.

[2] Delay and magnitude may be uncorrelated so that four reinforcement experiences are available using two magnitudes and two delays. Partial reinforcement is a still more special case where delay and magnitude are correlated providing only two reinforcement experiences.

Varied Delay and Magnitude Experiments I and Varied Magnitude Experiment II

Method

Subjects.—Subjects were black-hooded rats obtained from the Sennett Antique and Pet Shop, Tiffin, Ohio. They were 90–120 days of age at the beginning of their training.

Apparatus.—The double-alley maze has been described in detail elsewhere (**8**). For the present design, *S*s ran down a single straight alley, from which were automatically recorded starting speed (reciprocal of time to 6 in. from start door) and running speed (4 times reciprocal of time over 4 ft. of straight alley).

Training procedure.—Routine habituation and preliminary training procedures were employed (**8**). The *S*s lived in individual cages, were maintained on a 24-hr. hunger cycle, and were run one trial per day.

Experimental design.—During acquisition *S*s received the following conditions of reinforcement. (A) Varied Delay Exp. I (all *S*s received a constant 4-pellet magnitude of reinforcement): (1) 1-sec. delay (preferred delay group, $N = 8$); (2) 5-sec. delay (mean delay group, $N = 8$); (3) 1-sec. delay on a random half of each block of 10 trials, and a 9-sec. delay on the remaining half (varied delay group, $N = 7$). (B) Varied Magnitude Exp. I (all *S*s received a constant 4-sec. delay of reinforcement): (1) 9-pellet magnitude (preferred magnitude group, $N = 8$); (2) 5-pellet magnitude (mean magnitude group, $N = 8$); (3) 9-pellet magnitude on a random half of each block of 10 trials and a 1-pellet magnitude on the remaining half (varied magnitude group, $N = 8$). (C) Varied Magnitude Exp. II (all *S*s received a constant 4-sec. delay of reinforcement): (1) 9-pellet magnitude (preferred magnitude group, $N = 8$); (2) 9-pellet magnitude on a random half of each block of 10 trials and nothing on the remaining half (partial reinforcement group, $N = 8$); (3) 9-pellet magnitude on a random half of each block of 10 trials and a 1-pellet magnitude on the remaining half (varied magnitude group, $N = 8$).

The above conditions were run for 50 trials for Varied Delay Exp. I, and 60 trials for the two varied magnitude experiments. The *S*s in Varied Delay Exp. I were then given 20 nonreinforced trials; *S*s in Varied Magnitude Exp. I were given 30 nonreinforced trials; and *S*s in Varied Magnitude Exp. II were given 45 nonreinforced trials.

Extinction measure.—The most generally useful extinction measure would be one approximating the parameter of a decay function which determines the fractional decrement per extinction trial. When extinction curves begin at markedly different levels, the raw speed data do not adequately provide graphic representation or statistical test of possible differences in this measure. The data were therefore transformed to yield a direct indication of *relative rate* of extinction.

For each *S* individually, estimates of his terminal acquisition and extinction speeds were obtained; terminal acquisition speed was assigned a value of unity, terminal extinction speed was set equal to zero. Speeds during extinction were then expressed proportionately on this scale; e.g., for an *S* with a terminal acquisition speed of .6 and a terminal extinction speed of .2, a speed of .6 was rescored 1.0, one of .5 became .75, etc. Medians of these transformed scores were then taken and are presented in this paper. These operations, it will be seen, force all curves to begin at unity and approach zero, with the score showing the average percentage of loss yet to be demonstrated, and with the curves therefore suggesting the relative rate at which extinction is progressing.

Results

Performance during extinction of the varied delay group relative to its two control groups in Varied Delay Exp. I is shown in Fig. 1. No difference in relative rate of extinction is apparent among these three groups. Results of the Varied Magnitude Exp. I are not shown graphically. The two control groups did not appear to differ in relative rate of extinction, but there was a statistically unreliable suggestion that the varied magnitude group exhibited a slower relative extinction rate. The Varied Magnitude

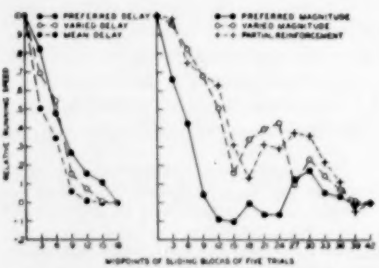

FIG. 1. Relative running speeds during extinction are shown on the left for Varied Delay Exp. I, and on the right for Varied Magnitude Exp. II.

Exp. II was intended to re-evaluate the suggested reduced rate of extinction following varied magnitude training, and to provide a partial reinforcement control group. Relative extinction curves of these three groups are shown in Fig. 1. It will be noted that the varied magnitude and partial reinforcement groups do not appear to differ in relative rate of extinction but both show clearly slower extinction rates than the preferred magnitude (continuous) control group.[3]

Statistical evaluation of these results was obtained by first finding for each *S* his median relative speed for the first five blocks of extinction trials. The 24 *S*s in each experiment were ranked and the Kruskal and Wallis *H* test (**3,** p. 423) was used as a test of over-all differences among the three groups. This test failed to reach conventional levels of statistical significance for Varied Delay Exp. I and for Varied Magnitude Exp. I. For Varied Magnitude Exp. II, $H = 13.528$ which, with 2 *df*, is significant at beyond the .01 level. The White T' (**3,** p. 417) test was then used to evaluate separately differences among the groups in Varied Magnitude Exp. II. The partial and varied magnitude groups did not differ significantly; between the partial and preferred magnitude groups, $T' = 39$ ($n_1 = n_2 = 8$, $P < .01$); and between the varied and preferred magnitude groups, $T' = 46$ ($n_1 = n_2 = 8$, $P < .05$). If the varied and preferred magnitude groups from the two varied magnitude experiments are treated together, $T' = 204$ ($n_1 = n_2 = 16$, $P < .05$).

The data were also analyzed in terms of the number of extinction trials required to reach a criterion of three successive trials with relative speed at or below .50 (i.e., to where extinction had progressed half as far as it was going to for that *S*). Identical levels of significance were obtained.

Discussion

Without here evaluating other published alternatives, we offer the following analysis concerning experimental extinction. The extinction of an instrumental response is due, at least in part, to the loss of incentive motivation. Incentive motivation in Hull's system (**4**) is labeled *K*, and following Spence (**11, 12**), *K* is assumed to be dependent upon the fractional anticipatory goal response (r_g). The extinction of an instrumental response is therefore related to the extinction of r_g which is, in turn, a function of the number of unreinforced evocations of r_g. If the conditioned anticipatory r_g on any trial is larger than would be appropriate for the primary reward actually given on that trial (i.e., a change from a larger to a smaller, or to no reward), the surplus r_g may occur several times without being reinforced. Following this kind of analysis, we propose the hypothesis that the loss of an instrumental response tendency which results from a nonreinforced trial is directly related to the extent to which r_g persists in occurring after the time of reinforcement has passed.

From this assumption, one would expect resistance to extinction to be greatest for *S*s who, immediately upon failure to receive an accustomed reward, stop performing the anticipatory goal response, or in subjective terminology, immediately stop anticipating reward. We shall call the cues which occur after the regular time of reinforcement on a nonreinforced trial the *postreinforcement-time cues*. Therefore, to predict relative resistance to extinction, one must determine whether the training conditions

[3] Since the graphs are presented in terms of relative extinction, readers interested in raw speed curves can approximate these from the following figures. Acquisition and extinction asymptotes were, respectively: preferred magnitude, .620 and .181; varied magnitude, .580 and .220; partial reinforcement, .537 and .222; preferred delay, .544 and .105; varied delay, .522 and .109; mean delay, .301 and .107.

allowed S to extinguish r_g to the postreinforcement-time cues, i.e., in everyday terminology, to take "No!" for an answer.[4]

Fortunately, employing this hypothesis within the Hullian framework permits a formal rather than a casual derivation of whether any specified reinforcement procedure provides this extinction experience. Since r_g is acquired and extinguished by the laws of classical conditioning, one has only to examine the training conditions from this point of view.

Consider first continuous reinforcement: r_g will become conditioned directly to the prereinforcement cues, but will also have a generalized tendency to occur to the postreinforcement-time cues. The latter do not actually occur during training because the reward itself, by stimulus interaction, modifies the on-going traces. (That is to say, the postreinforcement cues on rewarded trials are different from the postreinforcement-time cues on nonrewarded trials because they are changed by the occurrence of the goal stimulus.) When reward is omitted in extinction, however, the postreinforcement-time cues do occur, do evoke r_g, and thereby produce a large extinction effect.

Turning to varied delay of reward, the postreinforcement-time cues again do not occur on short delay trials. These cues do occur on long delay trials, however, and we must attend to the stimulus asynchronism between these postreinforcement-time cues and the subsequent reward. If the longer delay in varied delay is very long, then the occurrence of r_g to the postreinforcement-time cues will be nonreinforced because the subsequent reward is too late to act as an effective UCS. If, on the other hand, the longer delay is not very long, then the occurrence of r_g to the postreinforcement-time cues will be maintained by the later reward as an UCS. In general, the tendency for the postreinforcement-time cues to evoke r_g will vary inversely with the difference in delays used in varied delay training. Resistance to extinction will therefore be increased by varied delay training when the difference in delays is large, and will have progressively less effect as the difference becomes smaller.[5]

The effect of varied magnitude training will also depend upon the difference in magnitude. Varied magnitude training is assumed to involve two r_g's (that appropriate to the large, and that appropriate to the small, reward) both of which generalize to the postreinforcement-time cues and produce a compromise r_g intermediate between them. Thus, on small reward trials, there will be a generalized tendency for the compromise r_g to occur to the cues present after consuming the small reward, because these cues are similar to the prereinforcement cues. Since no subsequent large reward will occur to serve as an UCS, r_g will be extinguished to a stimulus pattern whose similarity to the postreinforcement-time cues depends upon the size of the small reward (because the smaller the reward the smaller the stimulus interaction with the on-going traces). Accordingly, the ex-

[4] This hypothesis is equally applicable to classical conditioning. In classical conditioning, the postreinforcement-time cues are the stimulus trace ages older than the age at which the UCS was previously given. Inhibition is a function of the number of unreinforced *responses*, but unless the response has been extinguished to the post-UCS cues, several unreinforced responses will occur on any one extinction *trial*. It should be observed that, using partial reinforcement, ages up to the time of the UCS receive 50% reinforcement to maintain the response, but post-UCS ages receive 100% nonreinforcement (because they occur only on the trials when the UCS is omitted) and the response becomes extinguished to them. As in an earlier analysis (7), there is set up a discrimination between the post-UCS ages and earlier ages. This hypothesis is not presumed, by itself, to account for all extinction results following various patterns of reinforcement. The Hull-V. Sheffield analysis is a logical consequent of the Hullian system and complements the present analysis.

[5] Varied delay may actually reduce resistance to extinction if the difference in delays is small because r_g then receives strong direct conditioning to the postreinforcement-time cues as well as the generalized tendency previously described.

tent to which the postreinforcement-time cues lose their tendency to evoke r_g (hence increasing resistance to extinction) will vary directly with the difference between the two magnitudes.[6]

These effects of varied delay and varied magnitude converge upon partial reinforcement as a limiting case in which the difference between the delays and the magnitudes is at a maximum. It therefore provides opportunity for the postreinforcement-time cues to lose their tendency to evoke r_g (because on some of the trials these cues occur without interaction by a small reward and without a later reward as an UCS), and result in increased resistance to extinction.

Varied Delay Experiment II

Of the predictions yielded by the above hypothesis, the one chosen for test involves an examination of the differences between a varied delay experiment reported by Crum, Brown, and Bitterman (1), in which varied delay produced an increased resistance to extinction, and Varied Delay Exp. I which failed to yield this result. There were three notable differences: (*a*) Their procedure involved massed training trials whereas we used widely distributed practice. (*b*) Their *S*s received reward immediately on half of the trials whereas ours waited 1 sec. on these trials. (*c*) Their *S*s were delayed 30 sec. on long delay trials, whereas ours waited only 9 sec.

Should a constant zero-delay group show the same rate of extinction as a 0–9 sec. varied delay group, using *massed* trials, we could conclude that varied delay does not always produce an increase in resistance to extinction even with massed practice and with an immediate reward on half of the trials. Such a finding would be consistent with the above r_g hypothesis, and would rule out the first two differences noted between previously reported results. However, a group with a large difference (0–30 sec.) in delays should, according to hypothesis, show a reduced rate of extinction.

Method

Apparatus and procedure were identical with the three earlier experiments except that massed training (15-sec. intertrial interval) was employed. The design involved three groups (all *S*s received a constant 1-pellet magnitude): (1) 0-sec. constant delay group ($N = 5$); (2) 0–9 sec. varied delay group ($N = 5$); (3) 0–30 sec. varied delay group ($N = 5$).

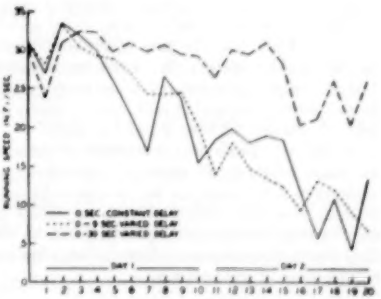

FIG. 2. Total running speeds during extinction for Varied Delay Exp. II.

[6] This hypothesis should not be confused with an assumption that the stimulus change produced by reward preserves learning *prior to* reward. If one chose to add the assumption that the occurrence of a nonrewarded r_g produces frustration, then the present hypothesis could be recast to describe extinction as a function of the frustration produced by nonreinforcement, with certain learning conditions providing extinction of this frustration. This added assumption does not appear to add anything to the particular problem here considered, and is therefore not introduced on the grounds of parsimony. Such an interpretation, however, may be contrasted with an hypothesis advanced by Wilson, Weiss, and Amsel (14) in which the frustration produced by nonreinforcement is presumed to increase during partial reinforcement training, to persist, and, as an associative variable, to bridge the time gap for application of the Hull-V. Sheffield kind of analysis.

Results and Discussion

The results are shown in Fig. 2. (Since some *Ss* did not continue running on all trials with massed extinction after the first two days, no reliable index of extinction asymptote could be obtained and raw speed curves are therefore shown.) Analysis of variance on the second day of extinction yielded an F of 15.745 which, with 2 and 12 df is significant at beyond the .001 level. Subsequent t tests show that the 0–30 sec. varied delay group was performing significantly ($P < .001$) better than either of the other two groups, which were not significantly different from each other.

The predictions based on the r_g analysis were clearly confirmed.

Summary

This article reports a series of experiments in which were separately observed the effects of varied delay and varied magnitude of reinforcement upon resistance to experimental extinction. The results of these experiments suggest:

1. When delay of reinforcement is varied between two equally likely values, under either massed or widely distributed practice, rate of extinction is decreased only if the difference in delays is fairly large. Specifically, variation between 0- and 30-sec. delay increased resistance to extinction whereas variation between 0 and 9 sec. and between 1 and 9 sec. did not.

2. When magnitude of reinforcement is varied between two equally likely values, under widely distributed practice, the rate of extinction may be decreased. Specifically, variation between 1 and 9 pellets increased resistance to extinction to an extent comparable with partial reinforcement (variation between 0 and 9 pellets).

3. An hypothesis was advanced to account for these and related findings. Resistance to extinction is assumed to be increased by any training procedure which permits extinction of the fractional anticipatory goal response to the cues which occur on nonreinforced trials after the regular time of reinforcement.

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REDUCTION OF SECONDARY REWARD VALUE AS A FUNCTION OF DRIVE STRENGTH DURING LATENT EXTINCTION [1]

HOWARD MOLTZ AND SALVATORE R. MADDI

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Several investigators (**2, 6, 7, 9**) have recently reported that certain pre-extinction procedures will weaken the strength of a previously learned response without that response itself being performed. These pre-extinction procedures were designed to "inform" *S* that food was no longer present in the previously baited goal box. This was accomplished by placing *S* directly in the empty goal box for a period judged sufficient to allow him to perceive its present non-rewarding character. The effect was observed when *S* was subsequently run to extinction and its performance compared with that of a control group that did not receive pre-extinction training. Some investigators (**1, 8**) have not found pre-extinction training to be effective in producing extinction. Nevertheless, it seems that, in some situations at least, performance is *not* a necessary condition for extinction.

An expectancy theory encounters no difficulty in providing an interpretation. Tolman has stated that "The CR is a response to the sign relationship $S_2 \rightarrow S_1$. And . . . [the response] goes off only so long as that sign relationship is 'believed' in by *O*" (**12,** p. 330). Thus, permitting *S* ample opportunity to observe that the reward object is no longer present in the goal location should be sufficient to weaken the expectancy of finding the reward object there again, so that subsequently *S* should require fewer trials to reach an extinction criterion.

Hull's theory of extinction, on the other hand, appears to encounter difficulty in providing an interpretation, since reactive inhibition and conditioned inhibition could not develop unless the response, or some component of the response, was performed. However, in a recent study Moltz (**6**) has contended that while Hull's response-induced inhibition theory requires the performance of the to-be-extinguished response, Hull's general behavior theory makes no such requirement. In line with this contention Moltz advanced the hypothesis that, following training in a T maze, if *S* were placed directly in the goal box without the presence of the reward object (a procedure which he called "latent extinction") this would reduce or eliminate the secondary reward value of cues present in the goal box, and that this would, in turn, lead to extinction through non-reinforcement or a decrease in reinforcement. Analysis of the data revealed latent extinction to be effective only when it resulted in a significant decrease in the secondary reward value of goal-box cues.

Hull (**3**) has suggested that the acquisition of secondary reward properties by a neutral stimulus is dependent upon that stimulus having conditioned to it some fractional component of the goal response (r_g)

[1] This article is based on the master's thesis of the second author conducted under the supervision of the first author. The first author is responsible for the design and for the manuscript in its present form.

and, in addition, that the stimulus will retain its secondary reward value as long as it retains the power to evoke r_g.[2] On this basis it would appear reasonable to expect that any procedure successful in reducing the response strength of r_g during latent extinction should result in the reduction or elimination of the secondary reward value of goal-box cues. Furthermore, the greater the reduction in the response strength of r_g, the greater should be the reduction of secondary reward value. The question arises as to the type of experimental procedure that might be expected to effect a reduction in the response strength of r_g during latent extinction.

Hull (3), Spence (11), and others (5) have conceived the fractional anticipatory goal response to be a theoretical construct whose relation to a specified set of observable events follows the same behavioral laws as such molar responses as bar-pressing and salivation. Therefore, all those operations that are assumed to effect a reduction in the strength of a molar response should also effect a reduction in the strength of r_g. If this be the case, then it appears reasonable to expect that the greater the number of nonreinforced evocations of r_g during latent extinction the greater will be the reduction in response strength. In addition, since it has been shown that increasing the effective drive level will increase the rate of evocation of a molar response, it would also appear reasonable to expect that the higher the drive level during latent extinction the greater will be the number of nonreinforced evocations of r_g that will occur during this period.[3] With these assumptions in mind concerning the relation between drive level, r_g evocation, and secondary reward value, the hypothesis of the present experiment may now be stated as follows. The higher the drive level during latent extinction the greater will be the reduction in the secondary reward value of cues present in the goal box as indexed by their capacity to mediate the learning of a new response in the absence of primary reinforcement. If it can be shown that these cues had previously acquired secondary reward value, then the condition necessary for testing the present experimental hypothesis would be satisfied.

Method

Subjects and apparatus.—The *S*s were 60 experimentally naive, male albino rats of the Wistar strain. The ages of the *S*s ranged from 100 to 150 days at the beginning of the experiment.

The apparatus was a single-unit T maze and a straight alley. The starting box, arms, and stem of the maze were painted gray. The stem of the maze was 21 in. long and 5 in. wide. There was a distance of 46 in. from the outer end of the left goal box to the outer end of the right goal box. The roof of the maze was covered with .25-in. mesh hardware cloth. Gray cloth curtains were placed in front of the doors to the goal boxes. Guillotine doors of light sheet metal were operated by *E*. The straight alley was 24 in. long and 5 in. wide and was also covered with .25-in. mesh hardware cloth and painted gray. Four goal boxes were used, two of which were painted black and two of which were painted with black and white stripes. The goal boxes were identical in all other respects. The same goal boxes were used on both the maze and the straight alley. In

[2] This suggestion is based on the assumption that the characteristic interoceptive stimulation (s_g) produced by r_g comes to acquire secondary reward properties as a consequence of its close association with primary reward. Any neutral stimulus to which r_g subsequently becomes conditioned will, in turn, acquire reward value through the arousal of s_g.

[3] When applied to molar responses, these assumptions readily follow from Hull's postulate set (3). Also, Skinner (10) has presented empirical evidence supporting these assumptions with regard to bar-pressing of rats and Jenkins and Daugherty (4) with regard to the pecking response of pigeons.

order to be certain that all traces of food were eliminated on nonreinforced trials, the same black or striped goal box was always used during these trials, while another black or striped goal box was always used during the reinforced trials.

Several layers of cheesecloth completely surrounded the maze and the straight alley. The cheesecloth was attached to an unpainted wooden frame, 4 ft. wide and 3 ft. high. The frame was nailed to the table on which the apparatus was placed. The use of the cheesecloth was intended to eliminate as many extra-maze cues as possible and to provide a nearly uniform perceptual field. The only source of light was a 25-w. bulb placed 3.5 ft. above the apparatus.

Habituation.—Prior to training, all *Ss* were given five days of habituation to the feeding regimen of the experiment. During these five days, *Ss* were fed an unlimited amount of wet mash once daily for 1 hr.

Straight-alley training.—Beginning on Day 6, *Ss* were given 10 trials a day for four days in the straight alley. All *Ss* were under a 22-hr. hunger drive at the beginning of each day's run. On seven of the 10 trials of each day, *S* found Purina Checkers scattered over the floor of the goal box. On the remaining three trials of each day, *S* ran to an empty goal box. The sequence of reinforced and nonreinforced runs was determined randomly. The intertrial interval was always at least 10 min. with a modal interval of 12 min. All *Ss* remained in the goal box for 45 sec. following either a rewarded or nonrewarded run. One-half of the *Ss* ran to a black goal box on rewarded trials and to a striped goal box on nonrewarded trials; the remaining one-half ran to the striped and black goal boxes on rewarded and nonrewarded trials, respectively.

Latent extinction period.—Twenty-two hours after the last straight-alley trial, all *Ss* were assigned at random to two groups. The *Ss* of the experimental group were placed directly in the previously positive goal box (either black or striped) for four 1-min. periods without the presence of food (latent extinction). Each period was separated by approximately 10 min. The *Ss* of the control group were not placed in the goal box during the latent extinction period but were treated like the experimental *Ss* in all other respects.

The experimental and control *Ss* were further divided into three additional subgroups. Each subgroup consisted of 10 *Ss*. The *Ss* of one subgroup (0-hr. group) were maintained on an unlimited amount of food beginning with the termination of straight-alley training until the conclusion of the latent extinction period. The *Ss* of the second subgroup (22-hr. group) were maintained on the 22-hr. feeding schedule used during straight-alley training. The *Ss* of the remaining subgroup (44-hr. group) were not given their usual feeding following the last straight-alley trial so that at the beginning of the latent extinction period they had not been fed for approximately 44 hr. Thus, the experimental *Ss* were given latent extinction under either 0, 22, or 44 hr. of hunger. The control *Ss* were also made differentially hungry during the latent extinction period but were not, of course, given latent extinction. Immediately following the latent extinction period, all *Ss* were given an unlimited amount of wet mash for 1 hr. Thereafter, food was withdrawn so that at the beginning of the test trials on the following day each *S* was again under a 22-hr. hunger drive.

Test trials.—The test trials were begun about 22 hr. after the last feeding period. They consisted of 15 free choices on the T maze. Food was not present in the maze during these trials. With the exception of the first test trial, one arm of the maze contained the previously positive goal box (either black or striped), while the other arm of the maze contained the previously negative goal box. On the first test trial, one-half of the *Ss* found the previously positive goal box whichever way they turned, while the remaining *Ss* found the previously negative goal box on whichever side they chose. This procedure was designed to assure a 50% base line on the initial test trial.

During testing, the intertrial interval was approximately 10 min. Correction or retracing after a response was not permitted. All *Ss* remained in the previously positive or previously negative goal box for 10 sec. following a choice.

For convenience, a response to the side of the maze containing the previously positive goal box will be referred to as a "correct response"; a response to the side of the maze containing the previously negative goal box, an "incorrect response."

Results

In order to test the present hypothesis, it was first necessary to demonstrate that the procedure employed during straight-alley training was adequate to establish the color of the positive goal box as a secondary reinforcer. A test of the adequacy of the training procedure in this respect was accomplished by comparing the mean number of correct responses made by the control *Ss* during the test trials against chance expectancy (7.50). It can be seen

TABLE 1

MEANS AND *SD*s OF THE NUMBER OF CORRECT RESPONSES FOR EACH GROUP

Conditions	Experimental *Ss*		Control *Ss*	
	Mean	*SD*	Mean	*SD*
0-hr. hunger	9.30	1.25	9.30	.95
22-hr. hunger	7.10	.99	9.90	1.37
44-hr. hunger	6.30	1.70	9.80	.79
Black goal box	7.60	2.26	9.87	1.13
Striped goal box	7.53	1.36	9.47	.99

from Table 1 that the mean number of correct responses for control *Ss* that were under either a 0-, 22-, or 44-hr. hunger drive during the latent extinction period was 9.30, 9.90, and 9.80, respectively. Each of these values exceeds chance expectancy at the .01 level. There can be little doubt that the training procedure employed in the present experiment was adequate to establish the color of the positive goal box as a secondary reinforcer.

In Tables 2 and 3 are summaries of the analysis of variance of the number of correct responses for the experimental and control *Ss* combined, and for the experimental *Ss* separately. The analysis of variance for the control *Ss* separately is not shown since neither the main effects nor the

TABLE 2

SUMMARY OF ANALYSIS OF VARIANCE OF THE NUMBER OF CORRECT RESPONSES FOR ALL *Ss*

Source	*df*	*MS*	*F*
Experimental, Control (E)	1	66.15	42.40*
Drive (D)	2	8.02	5.14*
Color (C)	1	.82	<1.00
E × D	2	17.15	10.99*
E × C	1	.42	<1.00
D × C	2	1.31	<1.00
E × D × C	2	.52	<1.00
Within cells	48	1.56	

* $P < .01$.

TABLE 3

ANALYSIS OF VARIANCE OF NUMBER OF CORRECT RESPONSES OF EXPERIMENTAL *Ss* ONLY

Source	*df*	*MS*	*F*
Drive	2	24.13	12.70*
Color	1	.04	
Drive × Color	2	1.73	
Within cells	24	1.90	

* $P < .01$.

interaction were significant. The trial-by-trial percentages of correct responses for all experimental *Ss* and for all control *Ss* are shown in Table 4.

Before testing the present experimental hypothesis it is also necessary to demonstrate that the latent extinction procedure was effective in reducing the secondary reward value of the color of the positive goal box. The mean number of correct responses of the experimental *Ss* was 7.57 as compared with 9.67 for the control *Ss* ($P < .01$, Table 2). The mean number of correct choices of the experimental *Ss* did not differ significantly from chance expectancy ($t = .21$; $P > .80$), while the mean number of correct choices of the control *Ss* was significantly in excess of chance expectancy ($t = 11.42$; $P < .01$). It appears reasonable to conclude that latent extinction was effective in reducing the secondary reward value of the color of the positive goal box as evidenced by the failure of the latter subsequently to mediate the learning of a new response. Whether *S* was rewarded in the black goal box or in the striped goal box during straight-alley training did not differentially affect the number of correct responses made during the test trials.

As shown in Table 1, the mean number of correct choices made during the test trials for experimental *Ss* given latent extinction under

TABLE 4

PERCENTAGE OF CORRECT RESPONSES

Groups	Trials														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Experimental	50	53	43	46	46	46	60	43	50	50	36	46	73	43	66
Control	50	80	70	70	80	76	63	63	53	63	60	53	56	50	76

0-, 22- or 44-hr. hunger drive was 9.30, 7.10, and 6.30, respectively ($P < .01$, Table 3). The values of t obtained by comparing the 0-, 22-, and 44-hr. experimental subgroups indicated that the 0-hr. subgroup made a significantly greater mean number of correct choices than either the 22-hr. subgroup ($t = 4.35$; $P < .01$) or the 44-hr. subgroup ($t = 4.49$; $P < .01$). While the difference between the 22-hr. and the 44-hr. experimental subgroups was in the expected direction, the value of t obtained was not significant ($t = 1.28$; $.20 < P < .30$).[4]

Before the experimental hypothesis is confirmed, it is necessary to examine the number of correct responses made by the control *Ss* during the test trials. An analysis of variance (not shown) of the number of correct responses for the control *Ss* separately indicated that the variance between drive levels was smaller than the within-cells variance. Since, in addition, the interaction between experimental and control *Ss* on the one hand, and different drive levels on the other, was significant ($F = 10.99$; $P < .01$), it is concluded that differences in drive level during the latent extinction period did not significantly affect the number of correct choices of the control *Ss*. The results support the hypothesis that the higher the drive level during latent extinction, the greater the reduction in the secondary reward value of goal-box cues.

[4] The mean of 6.30, obtained by the 44-hr. subgroup is significantly lower than chance expectancy ($P < .05$). However, this value appears to have been unduly influenced by one low extreme score. When this score is disregarded, the mean becomes 6.70, which does not differ significantly from chance.

DISCUSSION

The present experimental hypothesis was generated from a consideration of the fractional anticipatory response mechanism. Hull has suggested that a stimulus that has acquired secondary reward value will retain that value as long as it retains the power to evoke some fractional component of the goal response (r_g). On this basis, it was assumed that a procedure designed to effect a greater reduction in the response strength of r_g during latent extinction should result in a greater reduction in secondary reward. It was reasoned that (*a*) the greater the number of nonreinforced evocations of r_g during latent extinction the greater will be the reduction in its response strength, and (*b*) the higher the drive level during latent extinction the greater will be the number of nonreinforced evocations of r_g that will occur during this period. It was found, in conformity to expectation, that the higher the drive level during latent extinction the greater the reduction in secondary reward value as indexed by the capacity of cues that had previously acquired this value to mediate the learning of a new response.

The present theoretical analysis suggests an interpretation of latent extinction that is consistent with an S-R reinforcement approach. This analysis does *not* require that a response be performed in order for the extinction of that response to occur. But while the latent extinction phenomenon can be

deduced from Hull's general behavior theory, it is clear that his response-induced inhibition theory is not adequate to mediate this deduction since it makes performance a necessary condition for extinction. This suggests that the response-induced inhibition theory is either inadequate to provide an explanation of extinction or that latent extinction represents a more complex phenomenon than that to which Hull applied his inhibition theory. But if it is maintained that the inhibition theory is applicable to one class of response-elimination phenomena but is not applicable to response-elimination phenomena of another class involving more complex behavior mechanisms, then it is necessary to specify the parameters that distinguish these classes. Until such specification is achieved, the inhibition theory must remain suspect as an explanation of extinction—whether that extinction is assumed to be of the "simple" or "complex" variety.

Summary

Sixty male albino rats were given 10 trials a day for four days in a straight alley while under a 22-hr. hunger drive. The *S*s were rewarded on seven of the 10 trials of each day but were not rewarded on the remaining three trials. One-half of the *S*s ran to a black goal box on rewarded trials and to a striped goal box on nonrewarded trials; the remaining one-half ran to the striped and black goal boxes on rewarded and nonrewarded trials, respectively.

About 22 hr. after the last straight-alley trial, the experimental *S*s were placed in the previously positive goal box without the presence of food for four 1-min. periods (latent extinction). The control *S*s were not given latent extinction. The experimental and control *S*s were divided into three additional subgroups. The subgroups were under either a 0-, 22-, or 44-hr. hunger drive at the beginning of the latent extinction period. On the day following the latent extinction period the test trials were begun. These trials consisted of 15 free choices on a T maze. One arm of the maze contained the previously positive goal box, while the other arm of the maze contained the previously negative goal box. All *S*s were again under a 22-hr. hunger drive. Food was not present in the maze during the test trials.

The results supported the hypothesis that the higher the drive level during latent extinction the greater will be the reduction in secondary reward value as indexed by the capacity of cues that had previously acquired this value to mediate the learning of a new response. The relation between the present experimental hypothesis and the fractional anticipatory response mechanism was discussed.

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SPACED AND MASSED PRACTICE WITH A METHODOLOGICAL CONSIDERATION OF AVOIDANCE CONDITIONING

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Several theoretical positions predict that the extinction of a conditioned response will proceed more rapidly with massed trials than with spaced trials. Such a prediction stems directly from Pavlov's (**12**) internal inhibition theory, Mowrer's (**8**) fatigue theory, Hull's (**4**) reactive inhibition theory, and Miller and Dollard's (**7**) fatigue theory of extinction. A review of the literature indicates less agreement as to the empirical result in any given situation. Thus, most earlier investigators (**2, 12, 13**) have concluded that extinction is faster when trials are massed than when they are spaced. V. F. Sheffield (**15**) found directly opposite results with rats trained to run down an alley to obtain food. Animals extinguished under the massed condition (one trial every 15 sec.) were found to extinguish less rapidly than animals extinguished under a spaced condition (one trial every 15 min.). After a critical review of the earlier studies (**2, 12, 13**) she suggested rather convincingly that the evidence for their conclusions was rather tenuous.

Numerous studies have demonstrated the methodological significance of any change in procedure occurring concomitantly with the instigation of extinction. In this respect avoidance training affords unique advantages over other types of conditioning. In the avoidance situation no objective change occurs between conditioning and extinction. This is in contrast to other types of conditioning in which the presence or absence of the UCS is immediately apparent. However, in order to utilize this advantage of avoidance training it is necessary to continue conditioning to a high degree. Avoidance conditioning differs from classical conditioning in that the UCS is not presented if the animal performs the conditioned response. Classical and instrumental conditioning are thus recognized as different phenomena, i.e., experimental results are restricted to the type of conditioning employed. However, instrumental avoidance learning is identical to classical conditioning during the early phase of learning. It is only after the animal learns to avoid the painful stimulus that the procedures diverge. In other words, avoidance conditioning per se begins with the first failure to get shocked, not with the first shock. The present study represents an attempt to develop a methodological procedure which can be used to establish an avoidance response to a high degree without requiring a disproportionate amount of time.

More specifically, the present study was undertaken to determine: (*a*) the relative effect of massed and spaced trials on resistance to extinction, and,

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(*b*) the methodological importance of greatly reducing the total amount of time required to condition and extinguish an avoidance response.

Method

Subjects.—Sixty rats obtained from the Sprague Dawley farms served as *Ss*. These *Ss* were 50–60 days old at the start of the experiment.

Apparatus.—The apparatus consisted of a modified Warner (**18**) two-compartment conditioning box. The top and both ends were painted white with one side of the conditioning compartment formed by a sheet of milk glass. The remaining side consisted of a specially constructed one-way vision screen. Constant illumination was provided by a 25-w. bulb located at each end of the apparatus behind the translucent milk glass. A 2-in. barrier of ¼-in. stainless-steel rods separated the two compartments. The top bar of the barrier was a ¼-in. stainless-steel rod mounted on bearings to reduce perching. An agastat time-delay relay was employed for automatic presentation of the shock stimulus (110 v. with 85,000 ohms in series with *S*). Additional details of the apparatus have been reported previously (**9**).

Procedure.—The conditioning trials for each daily test session were preceded by a 2-min. adaptation period followed by the presentation of a buzzer CS. The shock UCS was automatically delivered after 5 sec. if *S* failed to make the barrier-crossing response to the buzzer. The buzzer, or buzzer and shock, were terminated manually when *S* responded.

The 60 *Ss* were randomly assigned to one of three groups with the following experimental conditions: in Group I, *Ss* were conditioned with an average intertrial interval of 20 sec. for each test period; in Group II, the intertrial interval for the conditioning and extinction trials averaged 40 sec. for each test period; in Group III, *Ss* were given three test periods with an average intertrial interval of 60 sec., three test periods with an average intertrial interval of 50 sec., three test periods with a 40-sec. average, three with a 30-sec. average, and three final conditioning test periods with an average interval of 20 sec. Extinction for this group was completed with an average intertrial interval of 20 sec. for each test period. When the intertrial intervals during conditioning are totaled for the three groups, Group I had an average of 20 sec. between trials and Groups II and III had an average of 40 sec. between trials.

Ten conditioning trials were given daily for the 15-day conditioning period and the 15-day extinction phase. The study was completed in three replications. One animal in Group III died during the conditioning phase.

Results

The percentage frequency of CRs during conditioning is presented graphically in Fig. 1. The number of CRs by Group I *Ss* was significantly lower than Group II ($P < .01$) and Group III ($P < .01$) as tested with Fisher's exact test of significance (**1**). Groups II and III were not significantly different during the conditioning phase.

A criterion found to be highly satisfactory in previous studies is to exclude *Ss* failing to respond at or above the 70% level for three consecutive days during conditioning. Sixteen *Ss* in Group I failed to meet this criterion and no extinction data were obtained for this group. Four *Ss* in Group III failed to meet this criterion and six *Ss* in Group II were also excluded from the extinction measure. The Group I procedure produced significantly more nonlearners than Group II ($P < .01$) and Group III ($P < .01$) as tested with the chi-square test.

With the nonlearners excluded, no significant differences were found between Groups II and III for the conditioning phase. Median CRs

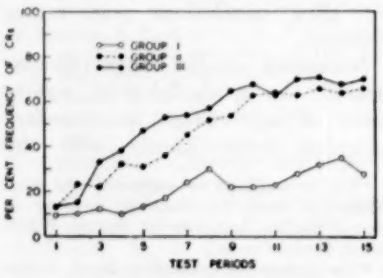

Fig. 1. The percentage frequency of CRs during the conditioning phase for all *Ss*.

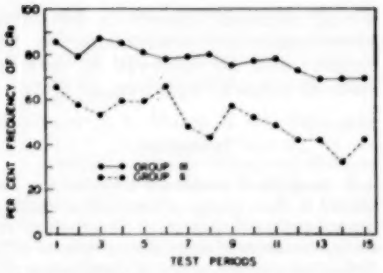

FIG. 2. The percentage frequency of CRs during extinction.

were 104.5 for Group III and 99.0 for Group II. Median CRs during extinction were 72.5 for Group II compared with 132.0 for Group III. The extinction data in terms of percentage frequency of response are portrayed in Fig. 2. The *Ss* conditioned with progressively shorter intervals between trials responded at a higher rate throughout extinction. This difference was significant at .003 with Fisher's exact test (**1**).

DISCUSSION

The present study was designed to provide basic data on the theoretical problem of the effect of massed vs. spaced trials on resistance to extinction. An additional aim was an attempt to solve the practical problem of greatly reducing the amount of time required to condition and extinguish an avoidance response.

Although the spacing variable was confounded somewhat by conditioning *Ss* under different time schedules, it appears clear that massing of trials did not lead to more rapid extinction as suggested by several theoretical positions (**4, 7, 8, 12**). The more surprising aspect is that the massing technique resulted in significantly increased resistance to extinction. This finding is similar to that reported by V. F. Sheffield (**15**) and attributed to the increased motivational effect generated by the frustrating omission of food during extinction. As Sheffield indicated, however, the frustration-induced motivation concept cannot be applied to situations in which noxious stimulation is omitted during extinction.

The data appear to conform with an ingenious hypothesis originally proposed by F. D. Sheffield and Temmer (**16**) and elaborated by Schoenfeld (**14**). Sheffield has suggested that an avoidance response persists for an unusually long time in comparison with other types of conditioning as a result of the training procedure. In the conditioning phase of avoidance training *S* is punished for making any response other than the arbitrarily designated CR. The patterns of response-produced stimuli accompanying responses other than the CR may, as Schoenfeld (**14**) suggests, acquire negative secondary reinforcement properties. During extinction the only response which does not give rise to the negative secondary reinforcement cues is the CR, and this response is highly resistant to extinction. This is really not surprising in that, as F. D. Sheffield notes, "Failure to respond in time is the only condition regularly reinforced, and all of the cue patterns signifying such failure are conditioned to the response in the course of avoidance training" (**16**, p. 288).

In a previous study (**10**) we have accepted the Guthrie (**3**) and Wendt (**19**) hypothesis that extinction results from the acquisition during extinction of responses which are incompatible with the conditioned response and lead to its eventual extinction. Applying the Guthrie-Wendt hypothesis in conjunction with the F. D. Sheffield hypothesis to the data of the experiment described herein provides an apparently parsimonious interpretation. In *Ss* with a longer time interval between extinction trials a greater number of incompatible responses occurred. These incompatible responses included face washing or grooming, sitting quietly, moving about the apparatus, etc. With a greater occurrence of incompatible responses, these *Ss* were given more opportunity

for the negative secondary characteristics of the cue patterns to extinguish. Accordingly, *S*s extinguished under the massed condition would receive less opportunity for extinction of the negative cue patterns accompanying incompatible responses and would be more resistant to extinction.

In addition, the punishment of incompatible responses was considerably different between groups during conditioning. As the intertrial interval for the massed group became shorter, fewer incompatible responses occurred, with the result that a greater proportion were contiguous with the fear accompanying buzzer presentation of the pain accompanying the presentation of shock.

From a methodological viewpoint the utilization of progressively diminishing intervals during the conditioning phase has the distinct advantage of continuing conditioning well beyond the classical-instrumental dichotomy without requiring a disproportionate amount of time. Such a procedure appears to provide an alternative to the type of study (**5, 6, 11**) which employs a low criterion of avoidance training and then attempts to draw generalizations which apply to avoidance conditioning generally. Although these studies (**5, 6, 11**) are empirically sound, it would appear desirable to continue avoidance training considerably past the point at which conditioning changes from a classical to an instrumental procedure.

Animals conditioned under a constant 20-sec. interval (Group I) were included in the study to anticipate the possibility that the advantages of the diminishing interval procedure could be obtained by starting *S*s directly on a 20-sec. schedule. The failure of *S*s learning under the Group I procedure to acquire the response was apparently attributable to the close temporal relationship of the CS with the UCS of the preceding trial. After *S*s had acquired the response, as in Group III, the UCS was seldom present and this factor played a comparatively minor role.

The technique of progressively shortening the intertrial interval as conditioning proceeds appears to have two advantages: (*a*) conditioning is enhanced; and (*b*) observations may be made on a much larger number of *S*s.

Summary

A conditioned avoidance response was developed in three groups of rats with a modified two-compartment apparatus. Progressively reducing the intertrial interval by a total of 67% distributed over the course of conditioning did not significantly alter the acquisition of the response. However, when *S*s were maintained throughout the course of conditioning on the reduced intertrial interval, learning was significantly retarded.

Animals conditioned under the progressively diminished interval procedure (intertrial intervals starting at 60 sec. and diminishing to 20 sec. during conditioning with extinction at a constant 20-sec. interval) were significantly more resistant to extinction than *S*s maintained on an average 40 sec. intertrial interval throughout conditioning and extinction. That is to say, progressively massing trials during conditioning with massed trials throughout extinction resulted in significantly increased resistance to extinction. It was noted that this finding is contrary to predictions from fatigue theories of extinction and an alternative interpretation was presented.

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DRIVE LEVEL AS A FACTOR IN DISTRIBUTION OF RESPONSES IN FIXED-INTERVAL REINFORCEMENT

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One of the ways in which reinforcement may be correlated with behavior is to make the former available as a periodic function of time. Practice on such a schedule in the Skinner box eventually leads to characteristic deviations from a constant rate of responding. The most prominent of these appear as depressions in rate directly following the delivery of a reinforcement and as subsequent increases later in the interval.

In his earlier formulation, derived from rat performance, Skinner (**6**) proposed that these deviations reflected a temporal discrimination arising from two circumstances: (*a*) responses which directly follow reinforcement are never reinforced; (*b*) responses which follow close upon other responses are seldom reinforced. The increase in rate which followed the postreinforcement depression was described as a compensatory effect. A more recent statement (**7**), utilizing data from the pigeon, attributes the depression in rate to a discrimination which is based on the cues produced during ingestion and asserts that once responding is resumed it is emitted at a constant rate. The last formulation vitiates the temporal discrimination properties of the phenomenon.

It has been found, however, that viewing these deviations as a temporal discrimination provides an interesting and useful index of the internal chemical state of an animal. A recent study (**8**) showed that the acuity with which the discrimination was performed, i.e., the degree to which responses tended to be concentrated toward the end of the interreinforcement interval, could be used as an index of drug-induced impairment. While there did not appear to be a relationship between the amount of impairment and appetite (as measured by food consumption), an experiment was undertaken to obtain more precise information on this question.[1]

Method

Sixteen male albino rats with experience in the Skinner box were used as *Ss*. After 250 min. of training on a fixed-interval reinforcement schedule ($i = 3$ min.), *Ss* were randomly assigned to two groups, with eight *Ss* per group. One group was put on a 22-hr. food deprivation program, the other on a 5-hr. deprivation program. Both groups were then given 150 min., spaced over five days, of further practice on the 3-min. fixed-interval schedule. The *Ss* were fed for 1 hr. daily.

The response distributions were obtained by projecting the records and counting the number of responses in each of the ten 18-sec. interreinforcement subintervals of the last five 3-min. intervals available from the 150-min. period.

Results

The data are shown graphically in Fig. 1. It can be seen that increased drive level produces an increase in response rate, the result typically obtained. It also appears that the number of responses per subinterval

[1] Sidman (**5**) has also found temporal discrimination to be a useful index of drug effects. His data, moreover, as do those reported in the present paper, suggest that there is no necessary relation between response rate and acuity of discrimination.

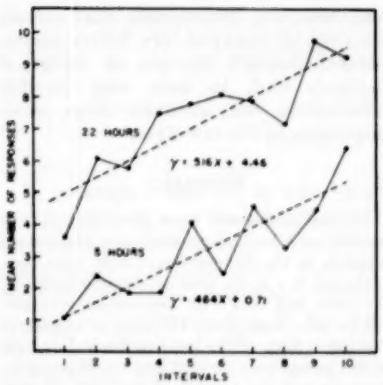

FIG. 1. Distribution of responses between reinforcements and least squares lines of best fit for both drive levels.

tends to rise as the end of the 3-min. interval is approached. Both of these impressions are supported by the results of the analysis of variance in Table 1.

The F ratio for Interval × Drive interaction indicates that the distribution of responses between reinforcements is not significantly affected by drive level. To determine the shape of this distribution, the contributions of the first- to the fifth-order components were obtained by means of the tables of orthogonal polynomials in Fisher and Yates (**2**). The results, as shown in Table 1, demonstrate that the preponderant contribution is made by the linear component. However, a significant increment in precision is afforded by the third-order component, indicating that a cubic equation provides the best fit of the function.

TABLE 1

ANALYSIS OF VARIANCE OF THE RESPONSE DISTRIBUTION DATA

Source	*df*	MS	*F*
Drive Levels	1	636.00	16.01**
Ss Alike	14	39.48	
Intervals	9	40.29	10.41**
Linear	1	304.03	78.56**
Quadratic	1	1.33	.34
Cubic	1	21.33	5.51*
4th Order	1	0.88	.23
5th Order	1	0.75	.19
I × D	9	5.10	1.32
Error	126	3.87	
Total	159		

* $P < .02$.
** $P < .01$.

DISCUSSION

These results impose two conclusions about the character of performance in fixed-interval reinforcement under the conditions of the present experiment.

The primary effect of drive level is on rate of responding. When increased, the principal result is to impel the organism to sample the environment more frequently; i.e., it uniformly lowers the threshold of response evocation through time. Moreover, if the distribution of responses is viewed as a temporal discrimination, then it can also be said that the acuity of this discrimination is unaffected by drive. Such a finding might be taken as a demonstration that the "quality" of performance is in some measure independent of drive level and appears related to the evidence that increased drive in maze learning produces a reduction in time but not in error scores (e.g., **3**). It is most closely linked to the study by Dinsmoor (**1**). Here, a discrimination situation was arranged so that reinforcement was available in the presence of a light (S^D) but was withheld during darkness (S^Δ). Under these conditions, variation in drive level (body-weight loss) led to changes only in the over-all response rate, and not in the ratio of S^D to S^Δ responses.

Response rate accelerates from the beginning to the end of the interreinforcement period. This contradicts Skinner's statement, based on pigeon data, that once the period of no responding which follows a reinforcement is ended, the organism thereafter responds at a constant rate. It is possible that the

contradiction arises from an interspecies difference which allows the rat to utilize cues correlated with time that are not available to the pigeon. For, when the pigeon is furnished with an external cue which corresponds to the passage of intra-interval time, the records (**7**, Fig. 4) then display the acceleration in rate found in the present study. This may indicate a deficiency in the pigeon's "clock," and suggests that distributions with even greater skewness may be produced by organisms phyletically superior to the rat.

Another way of looking at the distributions is to plot the mean interresponse latencies for each period. This procedure yields, in conformity with Mueller's model and data (**4**), an exponential function. The present results suggest that the exponential distribution of the interresponse latencies found by Mueller (who also used a 3-min. interval) may partly be due to the tendency to decrease these latencies as the end of the interval is approached, for Mueller assumes that responses are randomly distributed in time.

The present findings, together with those obtained before (**8**), suggest that the distribution of responses with a 3-min. interval tends to be mainly linear over a wide range of practice, since the earlier study provided twelve hours rather than the approximately four hours used in this experiment. It might also be noted that in this previous study no significant increase in the precision of fit was contributed by the quadratic and higher terms. This is not to say that more extended practice and other intervals will result in distributions of the same character. But it would be surprising if the main finding, i.e., the increase in rate as the end of the interval is approached, were to be jeopardized.

A final word is perhaps in order about the statistical approach to these data. Skinner (**6**, **7**) maintains that statistical methods have little to offer to a science of behavior because they obliterate the principles which typify individual performance. Studies such as the present one, however, demonstrate that certain features of behavior are better apprehended through the use of statistical methods and, in fact, may provide information not available from sheer inspection of the raw data.

Summary

Sixteen albino rats were given 250 min. of training on a 3-min. fixed-interval reinforcement schedule in the Skinner box. Half were then subjected to a 22-hr. food deprivation program, the other half to a 5-hr. deprivation program. All *S*s were then given 150 min. of additional practice. Each of the last five 3-min. intervals of this period were divided into ten 18-sec. subintervals and the number of responses in each subinterval counted. The results demonstrated that: (*a*) drive principally affects the rate of responding and does not alter the way in which responses are distributed in time between reinforcements; (*b*) response rate tends to increase from the beginning to the end of the interreinforcement period.

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AN INVESTIGATION OF THE PARAMETERS DEFINING DRIVE (*D*)

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In a recent study (1) it was found that groups of rats trained under 22 hr. and 4 hr. of food deprivation, in a black-white discrimination problem, did not differ from each other in trials, errors, or reinforcements to the criterion of learning. As a possible explanation certain aspects of the maintenance procedure were analyzed. It was shown that both groups were fed for 1 hr. during each 24-hr. period, although the 4-hr. group was run 4 hr. after feeding and the 22-hr. group, 22 hr. after feeding. This suggested that for an adequate definition of drive (*D*) more than hours of deprivation at the time of measurement need be specified. It was hypothesized that *D* is some function of at least two parameters, hours of deprivation at the time of measurement (h) and a second parameter corresponding to hours of deprivation during a unit of time (h_t). It was further hypothesized that h is related to stomach distension and its effects would therefore reach a maximum when the stomach was completely empty. There is some evidence that the emptying time of a rat's stomach is approximately 4 hr. (6). In the study mentioned, therefore, the 4-hr. and the 22-hr. groups were equated for both h_t and the effects of h. Although the groups were run, respectively, 4 hr. and 22 hr. after feeding, according to the present assumption, the component of *D* contributed by h was at or near its asymptote and, therefore, equal for both groups. Also, since both groups were deprived for 23 hr. out of each 24-hr. period, they were equated for h_t. On this basis no difference should have been expected in any measure reflecting *D*.

The present experiment was designed to determine whether the above *ad hoc* explanation had merit. It was hypothesized that groups of rats maintained under 47 hr. of deprivation out of 48 hr. ($h_t = 47$), and run 4 hr., 22 hr., and 45 hr. after feeding ($h = 4$, 22, and 45 hr.), respectively, would not differ in learning a brightness discrimination. Two additional groups, one deprived for 24 hr. out of 48 hr. and run 22 hr. after feeding and a second, deprived for 5 hr. out of 48 and run 4 hr. later were also studied. It was, therefore, predicted that these groups would display slower learning than the previous groups and that the latter of the two would be poorer than the former.

Using a binomial system of designation, the first number indicating the parameter h_t and the second the parameter h, the predictions may be summarized, conveniently, as follows: $(47/45 = 47/22 = 47/4) > 24/22 > 5/4$. A set of contrary predictions, would be yielded, however, if the more traditional definition of drive, viz., hours of deprivation at the time of measurement, were considered. These predictions, using the same system of designation, would be of the following order: $47/45 > (47/22 = 24/22) > (47/4 = 5/4)$.

METHOD

The apparatus and general procedure used in this study were described in detail elsewhere (1).

Only the necessary, abbreviated details will be presented here.

Subjects.—The *S*s were 45 naive, male, hooded rats between 120 and 150 days of age. They were divided, at random, into five groups of nine each. Three *S*s from one group and one from another died during the experiment leaving 9, 9, 9, 8, and 6 *S*s per group, respectively.

Apparatus.—The discrimination apparatus used in this experiment was adapted from Grice (4). Briefly, it consisted of a small, gray starting box opening into a gray, Y-shaped choice area from which *S* could enter either a black or white end box. The right-left placement of the black and white boxes was varied from trial to trial.

Conditions.—The five groups, mentioned above, differed with respect to their maintenance schedule. Three groups were fed for 1 hr. during each 48-hr. period (deprived for 47 hr. out of each 48-hr. period). They were run, 4 hr., 22 hr., and 45 hr. after feeding, respectively. The fourth group was deprived for 24 hr. out of each 48-hr. period and was run 22 hr. after feeding. The fifth group was deprived for 5 hr. out of 48 hr. and run 4 hr. after feeding. The groups were designated as Groups 47/4, 47/22, 47/45, 24/22, and 5/4, respectively, using the h_t/h system. Two weeks were allowed for adaptation to the maintenance schedule.[1]

Adaptation.—After *S*s were adapted to the maintenance schedule, adaptation to the apparatus was begun. During this period the black-white boxes were replaced by essentially identical gray boxes. One-half hour of group exploration, five *S*s at a time, was given for three experimental days. On the last day of group exploration the food cup in each box was filled with a quantity of food. During the remainder of the adaptation, *S*s were placed in the apparatus singly, and given, on respective days, 2, 3, 6, 6, and 6 free trials to a gradually decreasing amount of food. By the sixth adaptation trial they were running to one pellet, as was to be the case during training.

Training.—After Adaptation Trial 23, the black-white end boxes were substituted for the gray and only the white box was baited. This was done to correct for an initial black preference noted in pilot work. The *S*s were given six spaced trials per experimental day. On an incorrect run *S* was kept in the black box for approximately 20 sec. The criterion of learning was 14 out of 16 errorless trials.

Results

The mean number of errors, reinforcements, and trials to reach the criterion, for each group except Group 5/4 are presented in Table 1. The reason for omitting this group will be given below. The over-all difference among groups on each measure was tested using a simple analysis of variance. Since over-all differences for reinforcements and trials were significant beyond the .01 level, *t* tests were indicated. Group 24/22 differed from the others at beyond the .01 level for reinforcements. It differed from Group 47/4, for trials, at beyond the .02 level and from Groups 47/22 and 47/45 at beyond the .01 level. None of the differences between groups maintained under $h_t = 47$ were significant, regardless of the value of h. Over-all error differences were not significant, and therefore *t*s were not calculated.

Of the 9 *S*s in Group 5/4 ($h_t = 5$ and $h = 4$), 5 exceeded the latency criterion for exclusion from the experiment. The remaining *S*s responded correctly on so few trials that training was terminated after Trial 32. If all *S*s were to have run correctly from Trial 33 on, the mean trials to criterion could not have been less than 46 trials, well beyond the range of the other groups. This group then was considered to have shown the poorest learning of all.

[1] Motivational conditions in this study were specified solely in terms of the deprivation schedule. A record of the weight loss by groups, perhaps, would have been of additional value. Such data were not obtained. It is not likely, however, that the various *h*s had any differential effect on eating behavior. Consequently, the various groups in which h_t was 47 most probably sustained the same loss in weight. The $h_t = 24$, as well as the 5/4, groups had food in their cages for many hours during each 48-hr. period and certainly ate more than the groups in which h_t was 47. It is felt, therefore, that the comparisons among the three $h_t = 47$ groups and between them and the $h_t = 24$ group did not suffer as a result of the absence of such weight-loss data.

TABLE 1

MEAN ERRORS, REINFORCEMENTS, AND TRIALS TO THE CRITERION

Group	*N*	Errors		Reinforcements		Trials	
		Mean	*SD*	Mean	*SD*	Mean	*SD*
47/ 4	8	11.12	2.47	19.75	2.22	30.87	3.48
47/22	9	10.66	3.65	18.88	1.85	29.55	4.27
47/45	6	10.16	4.22	19.66	1.49	29.83	5.24
24/22	9	12.77	3.05	25.00	4.55	37.77	6.27

In summary, the order of learning for the groups, with respect to trials and reinforcements to the criterion, was as follows: 47/4 = 47/22 = 47/45 > 24/22 > 5/4. The Ss with high h_t required fewer trials and reinforcements to reach the same criterion than the lower h_t groups. The differences and lack of differences among groups were independent of the commonly used parameter h.

Although the groups did not differ with respect to mean errors to the criterion, there was a difference in the distribution of errors between the low h_t and the high h_t groups. In Table 2 the mean number of errors per group for Trials 1–12 and 13–24 are shown. It may be seen that Group 24/22 made fewer errors than the groups in which h_t was 47 during Trials 1–12, but more errors during Trials 13–24. An analysis of variance of the differences among the mean differences between Trials 1–12 and 13–24, gave $F = 4.18$ ($df = 3$ and 28; $P < .05$).

TABLE 2

ERRORS ON TRIALS 1–12 AND 13–24 FOR EACH GROUP.

Group	*N*	Trials 1–12		Trials 13–24	
		Mean	*SD*	Mean	*SD*
47/ 4	8	7.50	1.00	3.25	1.64
47/22	9	7.44	1.48	3.00	2.45
47/45	6	7.50	1.26	3.00	2.08
24/22	9	6.56	1.50	4.56	1.07

The intergroup differences between mean differences were tested with *t*. The differences between Group 24/22 and each of the $h_t = 47$ groups was significant at beyond the .02 level. None of the differences between any pair of groups in which h_t was 47 was significant, with *P* ranging between > .70 and > .90.

A further analysis of the mean errors during Trials 1–12 was possible. In a two-choice situation, such as the black-white discrimination problem of this study, the probability of an error is assumed to be .5. The expected mean number of errors in 12 trials, then, is 6. Using *t*, the mean errors per group was tested against this theoretical value. For Group 24/22, the mean errors during Trials 1–12 differed from the expected value at beyond the .30 level, but for Groups 47/4, 47/22, and 47/45 the significance levels were < .01, < .05, and < .05, respectively. The latter groups, then, made more errors on the first 12 trials than could reasonably be expected by chance, whereas Group 24/22 made no more errors than would be expected.

DISCUSSION

The results, with respect to trials and reinforcements to the criterion, were in line with predictions based upon a two-parameter definition of drive. This was not the case with respect to errors to the criterion. It was indicated, however,

that the groups did differ with respect to the distribution of errors. The high h_i groups made significantly more errors on Trials 1–12 than would be expected by chance, whereas the difference in the low h_i group was not significant. The difference in mean errors between Trials 1–12 and Trials 13–24 was also greater for the high h_i *S*s than for the low h_i *S*s. These differences in the pattern of errors can be explained by assuming that an incorrect habit was dominant at the beginning of training. It was noted above that there was an initial black preference. Thus, following Hull's assumption that performance is some multiplicative function of *D* and *H* (**5**), we would expect *S*s with high drive and dominant incorrect habits to commit more errors early in training than *S*s having equally dominant incorrect habits but low drive. Since in this study the *S*s were randomly assigned to the different groups it is a plausible assumption that the groups were equated with respect to correct and incorrect habit tendencies and the observed difference in the distribution of errors was due to the multiplication of equal *H*s by unequal *D*s. In this case the inequality in *D* could only have been determined by the different h_is. It is concluded therefore that the experimental hypothesis was confirmed and that an adequate definition of drive requires the specification of at least two parameters, h and h_i.

Taking into account the parameter h_i, several articles from the literature bear re-analysis. In Finan's paper (**3**) learning was studied as a function of four levels of drive. Groups were trained under 1, 12, 24, and 48 hr. of food deprivation. Animals were fed for 1 hr. each day and eventually trained and tested at that hour. The 1-hr. group received food 1 hr. earlier on the training day. The 12-hr. group was delayed in its feeding on the previous day and fed at a time such that 12 hr. intervened between the end of feeding and training. The 24-hr. group was fed at the regular hour on the day previous to training and the 48-hr. group was not fed at all on the day previous to training, thereby having 24 hr. and 48 hr. of deprivation previous to training, respectively. Extinction was under a uniform 24 hr. of deprivation. The major results were that the 1-hr. group was significantly less resistant to extinction than the other groups but none of the other intergroup differences was significant. It is clear from the results of the present study that all groups were under essentially $h_i = 24$ hr., and that only h varied at the time of training. Groups with an h of 12 hr., 24 hr., and 48 hr., respectively, should not have been different from each other and should have been superior to the $h = 1$ hr. group, as was the case.

In a similar study, Strassburger (**8**) examined learning under ½, 1, 4, 11, 23, and 47 hr. of food deprivation. His maintenance schedule was of a similar design to the one described above. The *S*s were kept under 22-hr. deprivation until the training day, at which point the schedule was interrupted to provide the desired levels of h. The ½-hr. group was fed sufficiently early to allow ½ hr. to intervene between the end of eating and conditioning. The 1-hr. group was similarly treated, etc. Extinction was under a uniform 23-hr. deprivation. Contrary to Finan, however, no differences were found even between the very low h and the high h groups. In terms of the present assumptions h should provide differential results as a function of variation below about 4 hr. There were, however, a number of other factors that may have been influential in obscuring any differences that may have developed as a function of conditioning under ½-hr. deprivation as compared with conditioning under 4 hr. and more. During adaptation all *S*s were under 22-hr. deprivation and received 40 pellets through the magazine of the Skinner box. If differences due to h were minimal, they could have been obscured due to the operation of secondary reinforcement, secondary drive, or some other irrelevant variable operative during extinction.

In a more recent paper Ramond (**7**) studied the learning of an instrumental response as a function of two levels of hunger deprivation and also delay of reinforcement. Only the former variable is of immediate interest. He too deprived both drive-level groups for 23 out of 24 hr. but tested one group 22 hr. and the other 4 hr. after feeding. With a single-tailed test P was .08 for the significance of the difference between groups. This was considered adequate for rejection of the null hypothesis. Under a two-tailed hypothesis or with a more rigorous criterion of significance ($P \leq .05$), however, the null hypothesis would have been accepted. It was presumably felt that the difference between high and low drive and its influence on learning was sufficiently well established that a less rigorous level of confidence could be accepted. In view of the present study this thinking becomes questionable.

Certain gaps, of course, remain in the present formulation. Whether the relationship between h and h_t is multiplicative or additive, etc., is still unknown. The various quantitative interrelationships between h and h_t and other constructs, such as D, ${}_sH_R$, and ${}_sE_R$ must be determined. And finally, it should be noted that the importance of h_t, or some similar factor, has long been assumed, although generally neglected in favor of h. This is indicated in a recent paper (2) in which it is asserted that a 24-hr. period of food deprivation has different effects on an S which has been on an ad libitum diet as compared with an S that has recently undergone prolonged deprivation. The importance of h_t is also implied in those studies in which drive is specified in terms of weight loss rather than hours of deprivation.

Summary

Forty-five rats (41 completed the experiment), divided into five groups, were run in a black-white discrimination problem. The five groups correspond to different degrees of food deprivation as follows: Group 5/4—deprived for 5 hr. out of every 48 hr. and run 4 hr. after feeding; Group 24/22—deprived for 24 hr. out of every 48 hr. and run 22 hr. after feeding; Group 47/4—deprived for 47 hr. out of every 48 hr. and run 4 hr. after feeding; Group 47/22—deprived for 47 hr. out of every 48 hr. and run 22 hr. after feeding; and Group 47/45—deprived for 47 hr. out of every 48 hr. and run 45 hr. after feeding.

It was hypothesized that D is some function of two parameters, h (related to hours of deprivation at the time of measurement) and h_t (related to total hours of deprivation during a unit of time). It was further assumed that h is related to stomach distension and that its contribution to D reaches an asymptote at about 4 hr. In this case, then, the only relevant parameter was h_t and it was predicted that learning facility would be in the order: $(47/4 = 47/22 = 47/45) > 24/22 > 5/4$. The experimental hypothesis was confirmed.

The implications of the parameter h_t and the necessity of specifying both h_t and the usual parameter h were shown by reference to the literature.

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IRRELEVANT OR PARTIALLY CORRELATED STIMULI IN DISCRIMINATION LEARNING[1]

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In this study the following four questions were investigated: What is the effect of (*a*) an irrelevant stimulus on discrimination learning, (*b*) the previous irrelevance of a stimulus on its subsequent discrimination, (*c*) a partially correlated stimulus on discrimination learning, and (*d*) previous partial correlation of a stimulus on its subsequent discrimination?

Experiment I dealt with the first two questions, and Exp. II with the latter two. Both experiments were intended as a basis for further evaluation of the continuity and noncontinuity theories of discrimination learning.

Experiment I

Method

Procedure.—The apparatus (see Fig. 1), which has been described elsewhere (**2**), was a four-unit linear maze with a starting box and a goal box. In each unit were two aluminum gates hinged at the top. Either gate could be locked by placing a nail behind it. Before each gate was a movable floor plaque. The bars on plaques were three-dimensional and painted gray.

The *S*s were 32 male albino rats, 110 days old at the outset of the study. They were adapted to the apparatus by a series of steps ending with a limited number of forced choices in which gray gates and plaques without bars were used. Self-correction of errors (defined as touching a gate with the nose) was permitted. At the end of the run *S* was allowed a few seconds to eat a wet mash of Purina Laboratory Chow in the goal box. Each group was fed once daily for 15 min. following the session.

Design.—The 32 rats were divided at random into an experimental (Group E) and a control (Group C) group. In Task 1, form discrimination was required (e.g., to approach longitudinal bars). Group E learned the form discrimination with color irrelevant, i.e., the positive form was paired randomly with black and white gates. The reason for using color rather than form as the irrelevant stimulus was that in this way it was certain that the rats were exposed to an irrelevant stimulus (black vs. white) which is highly distinctive and learnable for rats. Group C learned the form discrimination with color held constant (both gates gray). In each group, longitudinal was positive for half the rats, and lateral was positive for the other half. Both groups were given 128 trials (32 runs) on Task 1, which was sufficient for most *S*s to attain mastery.

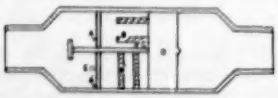

Fig. 1. Top view of unit of the four-unit linear maze. A and A′ are top-hinged aluminum gates. B and B′ are movable floor plaques. C is a partition, D is an air gap; and E a locking nail. Each unit is 28 in. long, 4 in. high, and 8 in. wide at widest part. Lateral and longitudinal bars are shown as hatched.

In Task 2, both groups[2] learned a simultaneous-type black-white discrimination without the forms being present. That is to say, gray floor plaques without bars were located before the gates. In each group, black was positive for half the *S*s, and white for the other half. There were 80 trials.

The stimulus orders were set up as follows: The positive stimulus appeared twice on the right and twice on the left in each run (four trials), and equally often on each side in each maze unit in each block of four runs. Where colors were paired with forms, in each run a

[1] The present study is part of a series comprising the "Cognition Project" under the direction of J. S. Bruner of Harvard University.

[2] The two slowest learners in each group were dropped at the end of Task 1 in order to increase the sensitivity of Task 2 comparisons. In addition, one rat had to be dropped from each group early in Task 2 because of "gate shyness," thus making 13 *S*s in each group.

given color was paired twice with each form. Within these restrictions, orders were random.

Results

Groups E and C, according to Fig. 2, learned form discrimination (Task 1) at about the same rate. Group E had a mean of 26.75 errors (SD = 9.97), while Group C had a mean of 31.69 (SD = 14.16). The difference between these means is not significant (t = 1.10).

Turning to Task 2, Groups E and C (see Fig. 3) learned color discrimination at about equal rates. Group E had a mean of 15.08 errors (SD = 3.99), while Group C had a mean of 12.92 (SD = 4.16). The difference in means is not significant (t = 1.30). Although the samples were small, there was little intragroup variability, so that a difference in means of only 3.43 would have been significant at the 5% level.

Experiment II

Method

The apparatus, methods of pretraining, and details of procedure were the same as in Exp. I. The *Ss* were 31 albino rats which were 110 days old at the outset of the study.

In Task 1 form was the critical variable. One color was paired with the positive form on 75% of the trials and with the negative form on the remaining 25% of the trials. For example, black would be the "75% color" if it were paired with the positive form on 75% of the trials, while white would be the "25% color," since it would be paired with the positive color on only

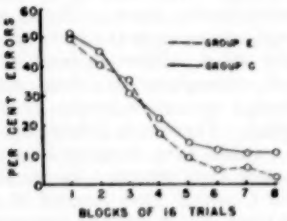

Fig. 2. Errors in Task 1 form discrimination.

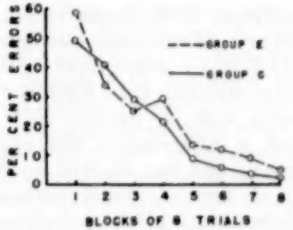

Fig. 3. Errors on Task 2 color discrimination.

25% of the trials. In Task 1 all 31 rats were given 128 trials (the same as in Exp. I.) This amount of practice was sufficient for an average of 95% correct responses on the last 16 trials of training.

In Task 2 color (black vs. white) was the critical variable, and form was held constant (longitudinal and lateral bars were absent). The 31 rats were divided at random into two groups. "Group 75" was reinforced for responding to the 75% color, i.e., the color which had been more often positive in Task 1, while "Group 25" was reinforced for responding to the 25% color (which had been more often negative in Task 1). The two groups were practically equivalent in the rate at which they had learned the previous discrimination. In each group, black had been the more frequently positive color for half the rats, and white for the other half. It was necessary to drop one rat from Group 75 and three rats from Group 25 because of "gate shyness," leaving 16 and 13 *Ss*, respectively. There were 80 trials.

Regarding stimulus orders, in Task 1 one color was paired three times with the positive form and once with the negative form in each run (four trials). In each set of four runs each color appeared equally often on the right and on the left. Other controls were similar to those of Exp. I, and within these restrictions orders were random.

Results

In Task 1 the mean of the errors was 25.85 (SD = 10.46). This mean was not significantly different from that of Group E (26.75) or that of Group C (31.69) in Exp. I.

In Task 1 there was a greater tendency to approach the negative form when it was paired with the 75% color than when it was paired with the 25% color (which was

negative on 75% of the trials). Of the 31 rats, 22 made more errors when the 75% color was paired with the negative stimulus than when the 25% stimulus was so paired; only four showed a difference in the opposite direction; and five showed no difference. A test of the hypothesis of equal frequency (13 in each direction) for the unambiguous cases yielded a chi square of 12.46 ($P < .01$).

In Task 2, there was an initial tendency to approach the color which had been more often positive in Task 1. In the first eight trials, 12 of the 13 rats in Group 25 more often approached the 75% color than the 25% color, and one rat showed no difference. Of the 16 rats in Group 75, 11 responded more often to the 75% color in these initial eight trials, two to the 25% color, and three showed no difference. Combining the groups, 23 *Ss* more often responded to the 75% color and only two to the 25% color. A test of the hypothesis of equal frequencies yielded a chi square of 17.64 ($P < .01$). In these initial eight trials, the mean percentage of responses to the 75% color was 70% in Group 75 and 74% in Group 25.

Figure 4 shows error curves for Groups 75 and 25 in Task 2. Group 75 had a mean of only 9.81 errors ($SD = 4.86$), while Group 25 had a mean of 27.15 errors ($SD = 5.79$). The difference in means is significant beyond the 1% level ($t = 8.50$). It should be noted that the Task 2 mean for Group E (15.08), in which color had been irrelevant in Task 1, fell between the corresponding means for Groups 75 and 25. The difference between the means of Group 75 and Group E is significant ($t = 3.23$, $P < .01$), as was the difference between the means of Group 25 and Group E ($t = 6.66$, $P < .01$).

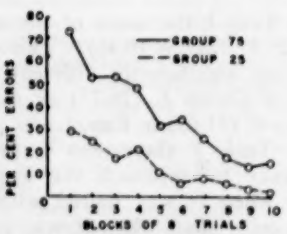

FIG. 4. Errors in Task 2 color discrimination.

DISCUSSION

Let us evaluate the continuity and noncontinuity theories of discrimination learning in regard to these results. Continuity theory postulates successive increments in the excitatory or inhibitory potential of a stimulus as a result of reinforcement and nonreinforcement, respectively. Presumably the components of a stimulus complex severally acquire response tendencies. Noncontinuity theory postulates an hypothesis-forming and hypothesis-testing process. Presumably each well-defined component of a stimulus complex would be eligible for a potential hypothesis. In the main, noncontinuity theorists such as Lashley (**4**) prefer to hold that only one hypothesis at a time is entertained, and that during such a period no learning occurs with respect to other components of the stimulus complex.

In Exp. I, Group E learned Task 1 as rapidly as did Group C, which shows that an irrelevant stimulus does not necessarily interfere with learning to discriminate a critical stimulus. A similar result was obtained by Elam and Bitterman (**3**). This finding is consistent with continuity theory. Since, on the average, responses to the two colors were equally often reinforced (and nonreinforced) throughout training, response tendencies to colors would have been balanced. Therefore, differential tendencies would have developed only with respect to form stimuli. Hence Groups *E* and *C* would have learned at about equal rates, which was the outcome.

But this finding is difficult to reconcile

with noncontinuity theory. Having had more dimensions of stimulus variation, Group E would have had more potential hypotheses (some erroneous) to form and test than would Group C (e.g., color and even joint color-form hypotheses). Whether only one hypothesis at a time were entertained, or several concurrently (a real feat), Group E on the average ought to have learned less rapidly then Group C, which was contrary to fact.

Turning to Task 2 of Exp. I, the fact that Groups E and C learned at about equal rates shows that the previous irrelevance of a stimulus does not necessarily affect its subsequent discrimination (when the previously critical stimulus is absent). This result is consistent with continuity theory. Since response tendencies to colors would have been equated on the average in both groups at the outset of Task 2, the two groups would acquire color discrimination at about equal rates. This finding poses a problem for noncontinuity theory, for it would appear that the previous irrelevance of a stimulus ought to render less probable an hypothesis about its subsequent relevance, and thus retard learning.

Regarding Exp. II, the fact that in Task 1 the *S*s were more likely to approach the negative form when paired with the 75% color than when paired with the 25% color follows directly from continuity theory. Responses having been more often reinforced (and less often nonreinforced) to one color than to the other, response tendencies to the 75% color would have been stronger than those to the 25% color. It is difficult to see how the one-hypothesis-at-a-time version of noncontinuity theory could account for this finding, especially after *S*s began responding at better than chance to form. The result may be reconciled with a more complex version which permits of multiple concurrent hypotheses at different orders of probability—an all-or-none hypothesis about form and a probabilistic hypothesis about color.

Turning to Task 2 of Exp. II, *S*s initially preferred the 75% color, and Group 75 learned more rapidly than Group 25, findings similar to those of Bitterman and Coate (1). Both findings follow directly from continuity theory, since, as explained previously, differential response tendencies to colors would have existed at the outset. A continuity view would attach no special significance to the fact that the mean initial percentage of responses to the 75% color was itself close to 75%. Since in Task 1 each alternative was only partially reinforced (and nonreinforced), habit strengths with respect to colors would have differentiated slowly. With more extended training on Task 1, the initial percentage of responses to the 75% color might have been higher in Task 2.

The foregoing results are difficult to reconcile with the version of noncontinuity theory which restricts learning to one hypothesis at a time. For if *S*s had emerged with a single form hypothesis in Task 1, what would account for the transfer to Task 2 of response tendencies to colors? If, however, noncontinuity theory were extended to allow not only multiple concurrent hypotheses, but also hypotheses at different orders of probability, then the initial tendency to approach the 75% color on nearly three-fourths of the trials would follow, and so would the advantage of Group 75 in learning Task 2. Such an elaboration of noncontinuity theory is speculative and requires much more evidence in its favor.

As a final comment, the results in Task 1 of Exp. I have implications for the perceptual nature of the stimuli. The stimulus materials appeared to function primarily as color and form *elements* (four in number). Such an assumption was employed in applying continuity theory to account for the fact that Groups E and C learned at about equal rates. If forms and colors had functioned primarily as color-form *compounds*, Group E, having had four such compounds, would have learned less rapidly than Group C, having had only two such compounds. If the colors and forms had functioned primarily as *"transverse patterns,"* then Group E, having had four such patterns, would have learned less rapidly than

Group C, which had only two patterns. Although learning might well have occurred on all three levels of stimulus complexity, in view of the outcome probably form and color elements were the dominant stimulus units.

Summary

Two experiments were undertaken to study discrimination of stimuli when one component is critical and the other is either irrelevant or partially correlated. In Exp. I Group E learned a form discrimination with color varying randomly, whereas Group C learned the form discrimination with color held constant. Both groups learned at about equal rates. Then each group learned the color discrimination (with forms absent). Again there was no difference. An irrelevant stimulus, therefore, does not necessarily interfere with learning to discriminate a critical stimulus, nor does its previous irrelevance subsequently affect its discrimination as a critical stimulus (the earlier critical stimulus being absent).

In Exp. II all rats learned the form discrimination while one color (the "75% color") was paired on 75% of the trials with the positive form. More errors occurred on trials in which 75% color was paired with the negative form than when the 25% color was paired with this form. Then one group learned to approach the 75% color while the other learned to approach the 25% color. Both groups showed an initial tendency to approach the 75% color, and Group 75 learned more rapidly than Group 25. Partial correlation of a stimulus, therefore does not necessarily interfere with or enhance learning to discriminate a critical stimulus, and the subsequent discrimination of such a stimulus depends on whether it previously had been more often positive or negative.

The implications of the findings for the continuity and noncontinuity theories of discrimination learning were stated. The continuity theory is favored by these data.

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THE EFFECT OF THE INITIAL REINFORCEMENT ON RESPONSE TENDENCY

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It is possible to reject a drive reduction theory of reinforcement while adhering to a position in which food for a hungry animal *brings about* the learning of an instrumental response (**2**). When we take this stand, there are three alternative explanations as to the nature of reinforcement.

One possibility is that the presence of a goal object (or its surrogate) confirms (or enhances) an organism's expectancy for this particular goal object. Such an interpretation has been suggested by White (**7**). According to this position, *no* instrumental-reward learning can occur until an expectancy for the goal object in the situation has *first* been established. In Hullian terms this viewpoint means that the translation of a fractional consummatory response (r_G) into a more intense r_G or the consummatory response (R_G) constitutes the mechanism of reinforcement.

The second possibility is simply that the eliciting of a consummatory response or its r_G strengthens a prior instrumental act, as seems to be implied by Sheffield and his associates (**5, 6**).

Both of these positions are in the "law of effect" tradition in that they imply that the reinforcing state of affairs strengthens the response tendency of a *preceding* instrumental response. The third possibility, on the other hand, does not involve the strengthening of a response which occurs *prior* to the reinforcing state of affairs. Reinforcement or the strengthening of any response tendency occurs (simultaneously) with the elicitation of the response in question. For example, the approach response characteristically elicited by food in a goal box is conditioned to contiguous stimuli; thus the rat learns to approach the correct side of a T maze (**2**). This third alternative is a contiguity notion in which a goal object is important to learning in terms of the kind of response it *consistently* elicits.

The present research was directed at differentiating between the first alternative and the last two, although the data appear to shed some light on distinguishing between the second and third alternatives as well. Thus, we need to answer the question, do *naive* animals show any learning on the very first rewarded trial. It might seem that this point could be determined by an examination of the literature. But preliminary training procedures as well as other methodological considerations obscure an adequate appraisal of this question. In order to test our hypothesis that the very first presentation of the goal object *can* contribute to the final response tendency, it is necessary to set up conditions for sensitive measurement of the effects of the first trial in a *differential response* situation.

Using a T maze and reversing the direction of the correct response after the first rewarded trial, Moore (**4**) made a first attempt at such a test.

[1] The experiment was conducted by R. L. Martindale as a requirement for the M.A. degree, 1950, Michigan State College.

The results, though clear, were confounded. The first reward contributed nothing while the second reward produced a significant increment to the response tendency. However, the goal object consisted of a single, small, familiar pellet of food in a glass coaster, and it was not uncommon for a naive rat on the first rewarded trial to take a minute or so to consume it. Since the animals initially had difficulty in finding the food and, therefore, *no immediate* "secondary reinforcement" through sight of the food, the experiment could not be considered an adequate test of the hypothesis.

In the present experiment special care was taken to insure immediate "secondary reinforcement" of the correct response. Also, the hypothesis was further evaluated by comparing the effect of the initial reward on naive animals and on animals which already had a food association established to the maze situation. If control of the variable of food expectancy is important, then the sophisticated animals should show more learning with the initial reinforcement than the naive.

Main Experiment

Method

Subjects.—The *S*s were 105 experimentally naive, male and female albino rats from the colony of the psychological laboratory of Michigan State University. Age varied from 60 to 160 days; the range and median age were practically identical for all conditions. Mean age was approximately 110 days at the beginning of the experiment.

Apparatus.—The enclosed T maze (Fig. 1) was designed to make the delay of reward as short as possible, as the response at the choice point was a continuous turn ending in the end box. The curved surfaces were constructed of heavy-gauge galvanized metal while the remainder was constructed of wood. All walls were 8 in. high. All the interior surfaces except the end boxes were painted a neutral gray.

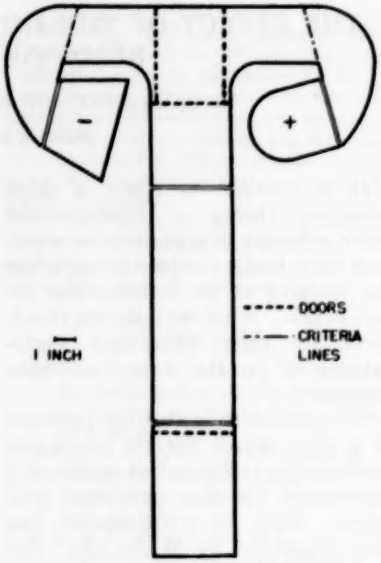

Fig. 1. Floor plan of the apparatus.

Except for the starting box, which had an opaque cover, the maze was covered with hardware cloth. The doors at the choice point were operated swiftly by a special release mechanism. When *S*'s nose crossed the response criterion line, the choice-point door and the door farther from the selected end box closed simultaneously, preventing any retracing or change of response. As soon as *S* had completely entered an end box, the door nearest the selected end box was also closed. The starting box had a conventional pull door, and all doors were padded at the bottom with sponge rubber.

There were two forcing blocks the same height and color as the rest of the maze which could be used to block off either alley at the choice point. The end boxes differed in shape as illustrated in Fig. 1. The negative end box was lined on the floor and sides with white posterboard in such a way as to leave a ¼-in. space between the posterboard and the wooden walls of the box. In order to control for food odor, this space was filled with crushed food identical to that used in the positive goal box. The positive goal box was lined with black posterboard and the entire floor was covered with Purina Checkers of the same type as *S*s were normally fed. Posterboard was used rather than paint to control for any differential paint odor. A large rectangular goal box was used in preliminary training with

certain groups. It was painted a neutral gray, covered with hardware cloth, and placed at the end of the straight alley.

The maze, in general, was lighted by a 200-w. bulb placed directly over the center of the straight alley, 68 in. from the alley floor. A 60-w. bulb with reflector was placed 4 in. from the top of the maze directly over each arm. Any illumination difference visible from the choice point was barely, if at all, discernible to *E*.

Habituation and preliminary training.—All *Ss* were given two days of habituation consisting of handling by *E* and running free on a small table. On the first day all *Ss* were given an unlimited amount of food in their home cage and on the second day were fed nothing. Water was available throughout the entire experiment. A 5-day preliminary training period followed in which the *Ss* were divided equally into a Food Expectancy (F) group and a No Food Expectancy (NF) group. The NF group was handled briefly by *E* and allowed to run free on a small table. These *Ss* were fed 7 g. of food the first day and 4½ g. on the remaining days of preliminary training. The *Ss* of the *F* group were placed in the starting box and allowed to run down the straightway to the preliminary training goal box which was littered with food, and each *S* was allowed 30 sec. eating time. The *S* received two massed trials per day and was fed 6½ g. the first day and 4 g. on each of the remaining four days. Obviously, a very high deprivation level prevailed for both groups by the end of this period. The *Ss* in both groups were fed in individual cages 10 min. after the end of the daily session.

Training.—Each preliminary training group was divided into three training conditions: Control (C), Experimental-1 (E-1), and Experimental-2 (E-2). This gave a total of six groups designated NF-C, NFE-1, NFE-2, F-C, FE-1, and FE-2. The control *Ss* were all trained consistently to one side of the T maze; for the E-1 groups the position of the positive and negative end boxes was reversed after one free and one forced trial of which only one was correct and rewarded; for the E-2 groups the position of the end boxes was reversed after two free and two forced trials of which only two were correct and rewarded.

The C and E-1 groups were equated for sex, initial position preference, and position of the positive end box. With 16 *Ss* in each subgroup this gave two *Ss* under each possible combination of conditions.

NFE-2 and FE-2 were equated for position preference only, but the other two variables were equated as much as possible. There were 14 *Ss* in each of these groups. Because of the equation of position preference on the basis of the first training trial most *Ss* were not assigned to a group until after the first trial. This technique made possible the beginning of all groups at the point of 50% correct response.

During training, *S* was given four trials per day for four days. Two of each day's trials were to the negative end box and two were to the positive box, thus each *S* had two reinforced and two nonreinforced trials per day.[3] The trials of the first day were alternately free and forced starting with a free trial. On succeeding days the first two were free and the second two forced, with only these combinations possible: LLRR, RRLL, RLLR, and LRRL. The *S* was rewarded with a 30-sec. eating period in the goal box. On trials to the positive side *S* was timed from the moment its nose touched the criterion line until it began to eat. The *S* remained in the negative end box 30 sec. after entering. The intertrial interval was approximately 30 min. All *Ss* were fed individually 10 min. after the last trial of the day. They were given 4 g. on the first two days and 5½ g. on the last two days. Thirteen discarded *Ss* were replaced with like animals giving a net *N* of 92 *Ss*. Animals were discarded for any one of the following reasons: refusal to eat during preliminary training on all trials of the first four days or on either trial of the fifth day, refusal to eat within 5 min. on the first trial of the training session, and refusal to eat on both rewarded trials of any subsequent day of training.

Results

The main measures employed were the number of correct responses on the free trials and the mean latency to eat after *S* touched the response criterion line. Wherever analysis of variance was used, the requirement for homogeneity was satisfied. An analysis

[3] Trials to the negative side were necessary in order to equate exploratory tendencies. When the first trial to the negative side followed a rewarded trial, it is conceivable that nonreinforcement of the incorrect response contributed to the tendency to make a correct response on the second free trial. This is unlikely, however, for various reasons. The third-order interaction (position preference × sex × position of end box) yields on *F* less than 1, and the available evidence (1) indicates that nonreinforcement of the wrong response influences the learning of the correct response only after learning has progressed a few trials and then only when the tendency to make a wrong response is fairly strong.

TABLE 1

NUMBER OF CORRECT RESPONSES FOR TOTAL TRAINING PERIOD

Groups	*N*	Mean	*SD*	Difference	*P*
Control (C)	32	7.12	1.05	C–E-1	.02
E-1	32	6.25	.87	C–E-2	.01
E-2	28	5.65	1.14	E-1–E-2	.10

of variance of the number of correct responses for the total training period indicates that there is no significant difference between preliminary training conditions ($F < 1$, $df = 1$ and 44) and a highly significant difference between training conditions ($F = 12.48$, $df = 2$ and 44, $P < .01$). Chi square for the largest single difference between preliminary training conditions on any one free trial, Trial 2, is also insignificant ($\chi^2 = 1.3$, $P = .30$). This is true even though the preliminary training variable was effective in producing significantly different latencies to eat, as will be shown later. Consequently, our hypothesis has received initial support and the preliminary training conditions were combined for the remaining analyses of learning. The mean number of correct responses for the combined groups for the total training period is presented in Table 1, together with *P* values.

The learning curves for the three training conditions are presented in Fig. 2. As apparent in Fig. 2, the effect of the initial reward is manifest on the second free trial. This increment is highly significant. On the second free trial 67 of 92 *Ss* repeated the response that was correct on the first rewarded trial ($\chi^2 = 9.18$, $P = .01$). The data clearly indicate that learning occurred on the basis of the initial reinforcement. A more complete analysis of the effect of the second reward would have been important if the initial reward had had no effect. However, since the E-2 data show that the second reward had an *additional* effect, they do provide further confirmation of our hypothesis.

Figure 3 shows the mean latency of the eating response on each of the training days for the two preliminary training conditions. In the analysis of variance the assumption of homogeneity was satisfied by using the

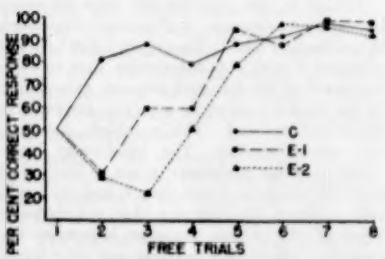

FIG. 2. Learning curves of the three training groups, preliminary training conditions combined.

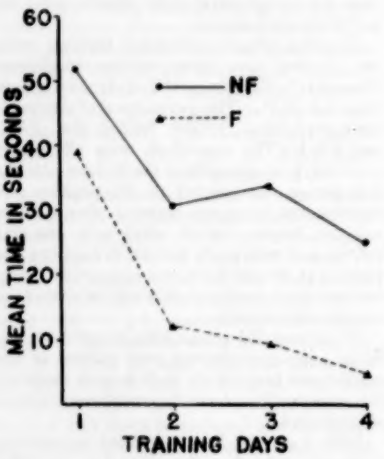

FIG. 3. Latency of the eating response during training for the two preliminary training conditions.

mean latency per day per animal rather than the mean latency over the total training period. The residual variance due to days was removed from the error term. The sophisticated F groups ate significantly sooner than the naive NF groups ($F = 6.31$, $df = 1$ and 86, $P = .02$), but there is no significant difference in eating latency between training conditions ($F < 1$, $df = 2$ and 86). The importance of this finding lies in the fact that this time difference does not appear to affect the speed of learning. Essentially the same number of *Ss* above (34) and below (33) the median eating latency of 48.5 sec. on the first trial went "correctly" on the next free trial. For the sophisticated control *Ss* the rank correlation coefficient between mean latency and number of correct responses for the total training period is .20 and for the naive controls is $-.02$. Both *Ps* fall far short of significance.

There exists a possible flaw in the experiment with respect to our interpretation of the learning in terms of elicited or reinforced *response*. It may be that the *making* of an *approach* response to the cues which immediately preceded reward was not an essential condition for the learning that took place on the first recorded trial. The learning may have been purely perceptual or cognitive, à la Tolman. For, due to the structure of the apparatus, when *S* was eating in the positive end box or alley many of the same stimuli, which were also visible from the choice point, were associated with eating and may have acquired approach or expectancy value. Thus the possibility remains that *S* responded correctly on the second free trial on the basis of the expectancy value of these cues rather than on the basis of having made a rewarded turn response. A subexperiment was designed to test this possibility.

Subexperiment

Method

The *Ss* were 12 male and 12 female albino rats, varying in age from 80 to 105 days. Through preliminary training, the procedure was exactly the same as for the naive groups of the main experiment. The training period consisted of a single day. The *S* was placed in the starting box and allowed to run down the stem to the closed choice-point door. Then *S* was lifted out of the stem by *E* and placed in either the positive or the negative end box. On this first trial, 12 *Ss*, 6 male and 6 female, were placed individually in the positive box with all doors closed. For 3 males and 3 females the positive box was on the right and for the other 6 it was on the left. The *S* was allowed to eat in the goal box for 30 sec. as in the main experiment. Approximately 30 min. later each *S* was given a similar trial except this time *S* was placed in the negative end box for a period of 30 sec. without food. Exactly the same procedure was followed with the other 12 *Ss* except that they were placed in the negative box first and the goal box second. Approximately 30 min. after the last training trial each *S* was given one free test trial in which the end boxes were in the same position as they had been during training.

Results

Ten of the 24 *Ss* went in the direction of the positive end box on the free trial. This proportion of correct responses is significantly smaller than 67 out of 92 on the comparable trial of the main experiment ($\chi^2 = 43.3$, $P = .001$). Therefore, there is no evidence that *Ss* learned the location of the goal box when the cues of the choice-point region were associated with food and eating on the first trial to the positive side in much the same way as occurred in the main experiment *after* *S* had arrived at the goal box. The conclusion of the main experiment need not be qualified.

Discussion

Since the present results support the hypothesis that the learning of an *instrumental* response can occur on the very first rewarded trial, they refute the

hypothesis that the reinforcement of an instrumental response is dependent upon the confirmation of a previously established expectancy.

Coupled with the fact that Moore (**4**), who used a single, small reward pellet, found no evidence of an increment from the first reward, the present results attest to the regular operation of some such principle as *immediate* secondary reinforcement in instrumental-reward learning. There is no basis in either study to claim the operation of such a principle as primary reinforcement. Without immediate "secondary reinforcement" there is no learning, and when there *is* learning, the latency of the eating response is long and unrelated to the speed of learning. Moreover, the large initial increment and the very rapid learning observed in the present experiment (essentially a 100% level of correct response after only five rewarded trials in all groups) attest to the immediacy and effectiveness of the "secondary reinforcement."

Also, the results of this study support a shift in reinforcement theory. For the results seem to offer an inkling as to the nature of reinforcement beyond what is indicated by the confirmation of the present hypothesis. Since the animals learn to eat in the goal box no faster than they learn the turn response, there is the suggestion here that the essential property of "secondary reinforcement," or reinforcement per se is an immediate approach to the food. In this way, instrumental-reward learning can be regarded as an example of classical conditioning. The unconditioned response is the response of approaching the goal object, and successive elements of this approach pattern become consistently and discriminatively conditioned to the cue stimuli of the maze (**2**).

Summary

In the main experiment, 92 rats served as *S*s in a simple T-maze learning situation in which the turn response was assured of immediate "secondary reinforcement." Half of the *S*s had a prior food association established to part of the maze and half were naive. All *S*s were given four trials a day for four days, two free and two forced, two reinforced and two nonreinforced. Position preference and the position of the positive end box were equated. On the second free trial there was a significant increment to the response tendency of the response which had been given one initial reward. There was no significant difference in the learning of the naive and sophisticated groups even though the latter ate significantly sooner than the former upon arrival at the goal box where the floor was littered with familiar food.

In a subexperiment 24 rats which had one trial to both the positive and negative sides, but were carried there instead of running there, did not learn.

The relevance of the results to reinforcement theory was discussed.

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THE ROLE OF EXPECTANCY IN DELAYED REINFORCEMENT [1]

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To a large extent, human behavior is characterized by apparent choices between immediate and delayed rewards and punishments. Reviews of animal learning studies (**4, 5, 8**) offer two findings: (*a*) immediate rewards are preferred over delayed rewards, and (*b*) the effectiveness of a reward or a punishment diminishes with increasing delay. These conclusions are supported by the comparatively little research on delayed rewards in human behavior. These studies dealt with performance on verbal mazes (**14**), classroom learning (**1, 12**), solution of conflict situations (**2, 3, 9, 10**), and selection of object preferences (**6, 7**).

The findings of delayed reward research have provided the basis for extensive clinical applications. Mowrer and Ullman (**11**) for example, considered the delay of rewards and punishments to be a critical factor in the development of neurotic and criminal behavior. Shaw has incorporated the idea of delayed reward into some proposed postulates of psychotherapy. He said (**15,** p. 426): "The problems dealt with in psychotherapy are in the nature of behavior which has immediately rewarding consequences, but more remote punishing consequences."

The proposed theoretical explanations of the two conclusions to delayed reward research have focused on the value of the reward.

They state, in effect, that a delay of a reward results in a decline of the value of that reward. With a decreased value, learning will be less effective and immediate rewards will be chosen over delayed rewards.

The present investigation was carried out within the framework of Rotter's social learning theory (**13**). This theory distinguishes between the *value* of a reward or reinforcement, and the *expectancy for occurrence* of the reinforcement. The definition for each of these two terms follows: "The reinforcement value of any external reinforcement may be ideally defined as the degree of preference for any reinforcement to occur if the possibilities of their occurring were all equal. . . . Expectancy may be defined as the probability held by the individual that a particular external reinforcement will occur as a function of a specific behavior on his part in a specific situation or situations" (**13,** p. 107).

In contrast to previous explanations, which focused on an hypothesized decline in the value of a delayed reward, the purpose of the present investigation is to introduce *expectancy* as a factor in delayed reinforcement situations. Since there are several meanings of "delayed reinforcement" and "delayed reinforcement situations," it should be emphasized that this study is concerned with human behavior in a situation involving an apparent choice

[1] This paper is based on a dissertation submitted in partial fulfillment of the requirements for the Ph.D. degree in Psychology at the Ohio State University. The author is especially grateful to Dr. Julian Rotter.

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between immediate and delayed reinforcements.

The present study hypothesized that in the course of previous experiences with delayed reinforcements, an individual will develop some level of probability (expectancy) that delayed reinforcements will occur. The level of the probability is presumed to be an important factor in the preference strength for the immediate or delayed reinforcement. Rotter (13) suggested that a decrease in the expectancy of occurrence of delayed reinforcements will result in a decrease in the strength of the preference for these reinforcements. The first purpose of this study was to develop varying levels of expectancy of occurrence of delayed reinforcements, in order to determine whether or not corresponding changes in the strength of the preference for delayed reinforcements, as compared with immediate reinforcements, would occur.

The second purpose was to investigate the extent to which training by one *E* generalized to a second *E*. It was hypothesized that social agents, perceived as instrumental to the occurrence or nonoccurrence of the delayed reinforcements, served as one of the important cues for the level of the expectancy. Different social agents would then be presumed to serve as cues for different levels of expectancy.

Hypotheses

When E_A offers the reinforcements there will be a regular decline in the choice of delayed reinforcements for high, moderate, and low expectancy groups respectively. Also, the high and low expectancy groups will have a respectively higher and lower choice of delayed reinforcements than will their respective control groups. In order to test the generalization of the effects of training, these hypotheses also apply to the situation where E_B offers the reinforcements. It is expected that there will be significant differences between E_A's subgroups and those of E_B.

Method

Subjects.—Forty-five *S*s were used in a preliminary research. There were 137 experimental and 97 control *S*s. These *S*s were matched, as far as feasible, in variables which appeared to be correlated with both the values of the reinforcements and the expectancy for occurrence of delayed reinforcements. These variables were: sex, age, grade, and socioeconomic status. All *S*s were males, 7 to 9, in Grades 2 and 3 of public schools located in a lower socioeconomic area of Columbus, Ohio.

Preliminary study.—A preliminary study was used to select two reinforcements which so differed in reinforcement value that approximately one-half of a group would choose the reinforcement of lower value when it was to be given to them immediately, and one-half of the group would choose the reinforcement of higher value when it was to be given to them a day later. With this pair of reinforcements, measurements could be made over a wide range of increases or decreases in the choice of reinforcements.

To select this pair, a measurement of the reinforcement value of a group of 11 toys was obtained by a rank-preference method. By progressively eliminating combinations of toys, a pair emerged in which half of the group chose the immediate reinforcement of lesser value, and half chose the delayed reinforcement of higher value. The immediate reinforcement was a small metal airplane, and the delayed reinforcement was a small, plastic "flying saucer." As a reliability check on the reinforcement values of these two toys, 25 additional *S*s chose the flying saucer (delayed choice) in preference to the airplane.

Experimental groups.—Three experimental groups were trained at three different schools, to prevent any effects of communication. To develop three levels of expectancy for the occurrence of delayed reinforcements, a series of five training trials was used. Following a preliminary activity, each *S* was told that he would receive a small balloon on the following day. This procedure was followed for five consecutive days, so that there was a total of four possible reinforcements. To develop the high, moderate,

and low expectancy for delayed reinforcements, four, two, and none of the promised reinforcements were received, respectively.

Three days following the training trials, each *S* was offered his choice of an immediate or delayed reinforcement. In doing this, each experimental group was divided into two subgroups. One of these subgroups was given the choice by E_A, who was the *E* during the training trials; the other subgroup was given the choice by E_B, who had no prior contact with the groups. To maximize the differences between E_A and E_B, (*a*) the *E*s differed in age and sex; (*b*) three days intervened between training and the choice situation; (*c*) no mention of the choice situation was made during the training; and (*d*) in the choice situation the two *E*s saw their *S*s simultaneously in different rooms so that communication between groups was prevented.

Instructions.—Each *S* was seen individually. On the first training day, for all *S*s, the *E* said:

"I am from a company that makes things for boys, and I am going to some of the schools. . . . Would you help me with these pictures? . . . These are coloring pictures. I want to know which is the best coloring picture . . . this one . . . or this one. Which is the best picture to color? . . . Thanks for helping me. . . . Do you like balloons? Would like me to bring you a balloon tomorrow? . . . All right, I will bring you a balloon tomorrow."

On the next training day, if *S* was to receive the promised reinforcement, *E* said:

"I promised you a balloon today, and here it is. . . . Would you help me with these two other pictures today? . . . Which is the best coloring picture?"

The previous instructions were followed, again promising a balloon the next day.

If *S* was not to receive the promised reinforcement, *E* said, "I am sorry, but I just don't have any balloons today. . . . " He then continued with the above instructions.

On the test day, the instructions given by E_A, who had trained all the experimental groups, were as follows:

"I told you that I was from a company that makes things for children. . . . Do you like toys? . . . Here are two toys. Do you know what they are? I am going to *give* you *one* of these toys. Would you like that? If you want this one (e.g., the airplane) you can have it *right now*. If you want *this one* (the flying saucer), it is the only one I brought, so you can have it *tomorrow*. And I will see your class tomorrow to see that each boy got what he chose. . . . Do you understand? . . . All right, which one will you choose?"

If necessary, these instructions were repeated.

The instructions given by E_B required an introduction, since this was the first contact with her subgroup: "Hello, my name is Mrs.———. What is your name? . . . I am here to see some of the children in your school. . . . Do you like toys? . . . " From this point on the instructions used by both *E*s were identical.

Control groups.—Control groups were used in order to control for possible differences in the expectancies for occurrence of delayed reinforcements prior to the training sequence. These groups were given the final choice of immediate or delayed reinforcements without any training sequence. One control group was used at each of the three schools, and each was given the choice of reinforcements by E_A. In order to investigate the generalization or specificity of the expectancy for delayed reinforcements, a fourth control group was used at an additional school. This group was given the choice of immediate or delayed reinforcements by E_B. To minimize and control any effects of communication, the control groups were seen two weeks prior to the beginning of training of the experimental groups at each school.

Results

Three control groups were given the choice of reinforcements by E_A. The chi-square values for comparisons among these groups were .205, .084, and .031. Since none of these values was statistically significant, these three groups were pooled, enabling chi-square tests to be made between experimental groups and the pooled control groups, and also among the experimental groups directly.

The basic data are given in Table 1 and in Fig. 1. A P value of .05 was taken as statistically significant. The high expectancy group had a significantly greater choice of delayed reinforcements than did either the moderate expectancy group ($.02 > P > .01$) or the low expectancy group ($P < .001$) when E_A offered the reinforcements. The moderate expectancy group had an insignificantly greater choice of delayed reinforcements than did the low expectancy

TABLE 1

CHOICES OF IMMEDIATE AND DELAYED REINFORCEMENT

Number Ss Choosing Reinforcement	Expectancy									Pooled Control A	Control B
	High			Moderate			Low				
	E_A	E_B	Control A	E_A	E_B	Control A	E_A	E_B	Control A		
Immediate	6	6	12	15	5	12	18	7	10	34	10
Delayed	19	14	11	11	15	13	6	15	12	36	16
Total	25	20	23	26	20	25	24	22	22	70	26

group ($.30 > P > .20$) when offered the reinforcements.

The high and low expectancy groups had a respectively greater ($.05 > P > .02$) and lower ($.02 > P > .01$) choice of delayed reinforcements than did their respective control groups, when E_A offered the reinforcements. There were no significant differences in the choice of delayed reinforcements by the high, moderate, and low expectancy groups when E_B offered the reinforcements ($.80 > P > .50$). There were no significant differences in the choice of delayed reinforcements by the high expectancy group and by its control group ($.70 > P > .50$) and by the low expectancy group and by its control group ($.70 > P > .50$), when E_B offered the reinforcements.

The low and moderate expectancy subgroups offered the choice by E_A, had a significantly lower choice of reinforcements than the respective subgroups, offered the choice by E_B ($P < .01$; $.05 > P > .02$). There were no significant differences between the respective high expectancy subgroups ($P = .30$). There were no significant differences between the pooled control groups, offered the choice by E_A, and the control group, offered the choice by E_B.

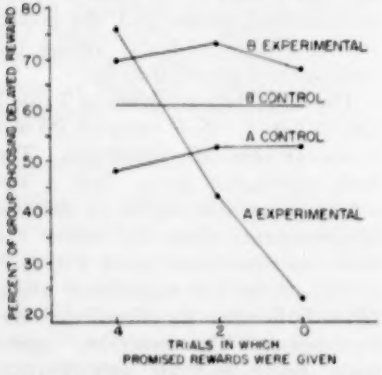

FIG. 1. Choice of immediate and delayed reinforcements by experimental and control Ss.

DISCUSSION

The results of this study suggest that a preference strength for a delayed reinforcement may be a direct function of the expectancy for delayed reinforcement. The apparent preference for immediate reinforcements, and the apparent decline in the effectiveness of a delayed reinforcement may, to a large extent, be a function of a lowered *expectancy*.

At least three implications may be drawn from these results. First, the effectiveness of a delayed reward or punishment may be increased, not necessarily by shortening the delay, but rather by increasing the expectancy that delayed reinforcements will occur. Sec-

ond, depending on the nature of the training, an increased delay may function as a cue for a higher, lower, or the same expectancy for the occurrence of delayed rewards. Third, the preference strength for delayed reinforcements may be increased or decreased by increasing or decreasing expectancy while the value of the reinforcement remains constant.

Generalization of expectancy.—The results indicated that the effects of training with E_A failed to generalize to E_B. Instead, there were uniform reactions to E_B independent of the kind of training with E_A. Furthermore, there were uniform reactions to E_B whether or not the *S*s had undergone prior training at all. Since the training was effective in raising or lowering the expectancies of the experimental groups, the lack of generalization may indicate that expectancies relating to E_B were already highly developed, so that training with E_A would fail to exert any appreciable influence on the expectancies relating to E_B.

According to this interpretation, behavior in a delayed reinforcement situation depends on the cues for various levels of expectancy for the occurrence of delayed reinforcements. Specifically, the results suggest that the two *E*s served as cues for different levels of expectancy. The implication is that delayed reinforcement behavior in general depends not only on the value of the reinforcements, but also on the expectancy for the occurrence of delayed reinforcements *as related to the social agent involved.* The effectiveness of a delayed reinforcement presumably will vary depending on the social agent who serves as the cue for the expectancy level.

Summary

This study introduces Rotter's expectancy construct as an important factor in delayed reinforcement situations. The hypotheses were: (*a*) other factors being equal, the preference strength for a delayed reward will be low, moderate, or high as the expectancy for the occurrence of the delayed rewards is respectively low, moderate, or high; (*b*) social agents (i.e., *E*s) will serve as cues for different levels of expectancy.

The *S*s were 294 second- and third-grade males, 7 to 9 yr. old. To select an immediate and delayed reward, a sequential analysis was made of the toy preferences of 70 *S*s. E_A then trained three experimental groups to develop high, moderate, and low expectancies for the occurrence of delayed rewards. Three days later, one-half of the experimental group was given a choice of an immediate or delayed reward by E_A, and one-half by E_B, with whom they had had no previous contact.

The results show:

1. Expectancy for delayed reinforcements may be developed or modified by a set of experiences in which delayed or future reinforcements either occur or do not occur.

2. In the development or modification of the expectancy for delayed reinforcement, if other factors are equal, the behavior potential for selecting delayed reinforcements will increase or decrease with a relative increase or decrease in the expectancy for those delayed reinforcements to occur.

3. In the psychological situation, one of the important cues for the level of the expectancy for delayed reinforcements is the social agent perceived to be related to the occurrence or non-occurrence of the delayed reinforcement. The results suggest that different social agents may serve as cues for different levels of expectancy of delayed reinforcement and, therefore, different behavior potentials for choosing delayed reinforcements over immediate ones.

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STIMULUS PRETRAINING AND SUBSEQUENT PERFORMANCE IN THE DELAYED REACTION EXPERIMENT

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Several investigators have reported studies of the delayed reaction in children (**1, 2, 6, 10, 11, 12, 14**), although much remains to be done in discovering the relevant experimental variables and in specifying the theoretical significance of the delayed reaction experiment itself. One of the factors that has been suggested by Hunter (**10, 11**) and by Skalet (**14**) as influencing the correct performance of children in the delayed reaction experiment, as well as in determining the length of delay that can be tolerated by *Ss* without interfering with the correctness of the performance, is the presence of symbolic or linguistic responses to represent the relevant stimulus features. Skalet reported that her child *Ss* could tolerate longer delays with stimuli for which the *Ss* had names (familiar animal forms) than for stimuli for which the *Ss* did not have names (geometrical figures). She concluded that the children's abilities to represent the animal forms with symbolic responses in some way facilitated recall following delay.

Although the writer is unaware of published studies of the relationship between stimulus similarity and the correctness of the delayed reaction or the length of delay that can be tolerated by *S*, this variable is undoubtedly relevant. In the extreme case, if identical stimuli were used, *S* would have no basis at all for a correct response following the delay. It is conceivable that the animal forms which Skalet used were less similar than the nonsense figures, and therefore that the better performance with the former is due to stimulus distinctiveness rather than to possession of names.

The present experiment is concerned with a more direct test than Skalet's of the hypothesis that the possession by children of names for the stimuli leads to more correct choices following a delay period. In the present design, half the *Ss* are taught different names for two similar stimuli in a pretraining session, while the other half are given discriminative experience with the same stimuli but without learning names for them. All *Ss* are then run in a delayed reaction situation involving these stimuli. Since older *Ss*, even without pretraining, are more likely than younger *Ss* to invent names or other descriptive phrases for the stimuli, the prior learning of names should be relatively more effective for younger *Ss* than for older *Ss*. Accordingly, each experimental group consisted of a younger and an older subgroup in order to study the interaction between age and type of pretraining.

METHOD

Apparatus.—The primary apparatus consisted of a round wooden disc, 18 in. in diameter, mounted on the shaft of an electric motor. Fastened to the top surface of the disc, one opposite the other, were two identical wooden

[1] The writer wishes to thank his research assistant, Miss Ruth B. Holton, for suggestions concerning experimental procedures and for aid in collecting the data.

boxes, 4 in. on each side, with hinged covers. The hinges were placed on the outsides of the boxes on the sides toward the center of the disc. The tops of the boxes were covered with a transparent acetate, under which the stimuli could be placed, and a wad of white cotton was put in the bottom of each box to prevent the rattling of the rewards placed in the boxes. A normally open push-button microswitch was wired into the circuit of the motor so that the motor could be started or stopped by pressing or releasing the switch. A section of rubber garden hose was mounted on the outer rim of the disc in order to protect *S* from the rotating disc and to provide *E* with a smooth area to grasp in stopping the disc. The entire apparatus was covered with a heavy coat of flat black paint.

There were two stimuli, each consisting of a set of black-lined concentric circles placed on white backgrounds 4 in. square. One of the stimuli (S-7) had 7 concentric circles, the other (S-5) had only 5. The inner circle in each stimulus was ¼ in. in diameter; the outer circle, 3½in. The remaining circles were spaced equidistant between the outer and inner circles. Thus, the two stimuli differed in the number and density of concentric circles. Two additional pairs of the stimuli, identical to those described above, were used in the pretraining session.

Experimental design.—The experiment was conducted in four distinct phases, all given to each *S* on the same day. In the first phase, the pretraining stage, the stimuli were presented two at a time and *Ss* in one group (Group N) were required to call S-5 by the name "five" and S-7 by the name "seven." The second group (Group D) was required to discriminate the stimuli by saying "same" when both stimuli were S-5 or S-7, and "different" when one stimulus was S-5 and the other S-7. In the second phase, *E* familiarized *Ss* with the delayed reaction experiment by putting an orange stimulus on one box and a blue stimulus on the other, baiting one of the boxes with a toy coin, spinning the disc briefly, and then asking *S* to find the coin. In the third phase, the test problem, the concentric circles were used as stimuli on the boxes, one box was baited each time, the disc was rotated rapidly for 25 sec., and *S* was then asked to find the coin. Finally, in a check phase, concentric circles were removed from the boxes, one of the boxes was baited, the disc was briefly rotated, and *S* was asked to find the coin. This last step was taken in order to control for the use of extraneous cues by *S*.

Subjects.—The *Ss* were 54 children from the Iowa Child Welfare Research Station preschool laboratories. They were divided into two age groups; the younger group of 26 *Ss* ranging from 3 yr., 9 mo. to 4 yr., 9 mo.; the older group of 28, from 4 yr., 10 mo. to 5 yr., 6 mo. One-half of each age group was randomly assigned to Group N, the other half to Group D. Seven additional *Ss* were eliminated from the experiment following failure to learn the pretraining task in the 72 trials allowed. Of these, 4 were in Group N and 3 were in Group D.

Most of the *Ss* had previous experience in name-learning experiments and about 12 had previous experience in a delayed reaction experiment. In neither case, however, were the stimuli and the names of the present experiment used.

Procedure.—The *S* was brought into the experimental room by a familiar *E*. The *E* lay out two pairs of the concentric circle stimuli on a table before *S*. Group N *Ss* were told that the name for S-5 was "five" and that the name for S-7 was "seven." The stimuli were then presented two at a time and *S* was informed that he could tell the difference between "seven" and "five" by the number of circles. The *Ss* were not urged to count, although a few *Ss* in both groups did count. Four different stimulus settings were used: in one, both stimuli were S-5; in a second, both were S-7; in a third, S-5 was on the left, S-7 was on the right; and in the fourth, S-7 was on the left, S-5 on the right. The stimuli were presented in a prearranged order such that in each successive block of four trials, each of the four settings appeared once. A trial was counted correct only if *S* named both stimuli correctly. Group D *Ss* were treated like those of Group N except that they were not told the names of the stimuli and their task was structured for them as saying "same" when both stimuli were S-5 or S-7, and "different" when one stimulus was S-5 and the other S-7. The stimuli were presented in the same order for Group D as for Group N. The *Ss* in both groups were continued in the first phase until they made correct responses on 12 consecutive trials, or until 72 trials had been given. In the latter case they were eliminated from the experiment.

In the familiarization phase, *S* was taken to the apparatus described above. A 4-in. square piece of orange construction paper was placed on top of one of the two boxes, and a similar piece of blue paper was placed on the other. While *S* watched, *E* dropped a toy coin onto the wad of cotton in the bottom of one of the boxes and closed the lid. The *E* then asked *S* which box contained the coin. If *S* responded correctly, *E* immediately spun the disc rapidly and stopped it. The time required to spin and stop the disc was ordinarily about 1 sec. When the disc was stopped, *S* was asked to find the coin. This procedure was repeated with the orange and blue stimuli, in an irregular alternation,

until *S* had made three consecutive correct responses.

Before beginning the test problem, *S* was told that he would now have an opportunity to win more toy coins, and that when he had won enough he would be able to buy a toy with them. The orange and blue stimuli were replaced with the concentric circles. Six trials were given to each *S* with the coin being placed in one of the two boxes in the order: S-5, S-7, S-7, S-5, S-7, and S-5. The *E* would bait the appropriate box for that trial and would then ask *S* to point to the baited box in order to assure that he had observed the baiting. Group N *Ss* were also asked to name the baited box. When *S* had responded correctly, *E* immediately spun the disc and kept it spinning for 25 sec. at a speed that made it impossible for *S* to follow the baited box visually. At the end of the 25-sec. period, *E* immediately stopped the disc and asked *S* to find the coin. The between-trial interval averaged approximately 1 min.

The check problem was given in order to determine whether or not *S*'s delayed reaction was based on extraneous cues. The discriminal stimuli were removed from the boxes and *S* was given 6 trials with a delay of about 1 sec. as in the second phase. The *Ss*' choices were recorded on prepared sheets throughout the delayed reaction phases of the experiment. The *Ss*' verbalizations regarding the stimuli were recorded throughout the experiment

Results

Pretraining.—The number of trials to the criterion of 12 consecutive correct responses was determined for each *S*. The means and *SDs* of these are presented in Table 1. It is apparent that both the younger and older *Ss* of Group D reached the criterion in fewer trials than did those of Group N. Despitè sizable differences in the means, a treatment by levels analysis of variance failed to show significant differences in the main effects for age or pretraining or in their interaction for either the raw data or a square-root transformation of the raw data ($P > .10$). With some caution, it may be assumed that the two pretraining groups had approximately equal experience with the stimuli during the pretraining task. In any event, all *Ss* had demonstrated discrimination between the stimuli at the same high response criterion.[2]

Only nine incorrect responses were made during the familiarization phase, and no *S* made more than two incorrect choices prior to reaching the criterion of three consecutive correct responses.

Test for extraneous cues.—The mean number of correct choices in the six-trial check for extraneous cues is presented in Table 1 for each group. A mean of 3.0 is expected on the basis of chance alone. It is apparent that none of the four groups differs markedly from the expected mean and this observation is borne out statistically in a treatment-by-levels analysis of variance. None of the main or simple effects is significant at less than the 10% level. Thus, it seems safe to assume that *Ss* were unable to respond correctly, even with short delay, without the use of the discriminal cues on which they had received pretraining.

Test phase.—Table 1 gives the means and *SDs* of the number of correct responses for the four groups during the critical test phase of the experiment. From this table, it is apparent that Group N, on the average, performs better than Group D; that the older *Ss* perform better than the younger *Ss*; and that the younger *Ss* in Group D perform considerably worse than *Ss* in any other subgroup. A treatment-by-levels analysis of these data is summarized in Table 2. The significant

[2] Correlations obtained between the number of pretraining trials to criterion and the number of correct responses on the test suggest that even if the difference were significant, it could be safely disregarded for present purposes. The Pearson correlation coefficients for the various groups were: D—O, − .05; N—O, − .05; D—Y, − .27; and N—Y, .03. The correlation for all *Ss* combined was − .17.

TABLE 1

MEANS AND *SD*s IN THE PRETRAINING, THE TEST PHASE, AND THE CHECK FOR EXTRANEOUS CUES

Group	*N*	Pretraining Phase: Trials to Criterion		Test Phase: Correct Choices		Extraneous Cue Check: Correct Choices	
		Mean	*SD*	Mean	*SD*	Mean	*SD*
D—Young	13	20.8	21.2	2.92	1.40	2.85	1.28
D—Old	14	14.8	16.7	5.00	0.96	3.36	1.34
N—Young	13	25.7	17.8	4.77	1.01	3.23	1.17
N—Old	14	25.8	20.1	5.14	1.10	2.79	1.26

interaction of age and pretraining suggests the need for an examination of the simple effects. The difference between means for Group N and D was evaluated by a *t* test separately for the older and younger groups. For the older *S*s, $t = .20$ and $P > .20$. For the younger *S*s, *t* was 3.96, $P < .001$. Thus, it appears that the effectiveness of the name-learning was restricted primarily to the younger *S*s.

Additional evidence for the relevance of names to the performance in the delayed reaction situation is provided by an examination of the records of *S*s in Group D. The *E* recorded verbalizations of 8 *S*s in this group who used discriminal names to refer to the concentric circles (e.g., "bigger one," "shiny one," "darker one," "wider one," etc.). The mean number of correct responses on the test for these *S*s was 5.38 and the mean for the remaining 19 was 3.42. The value of *t* obtained was 3.15 with $P < .005$.

TABLE 2

ANALYSIS OF VARIANCE OF THE NUMBER OF CORRECT CHOICES IN SIX-TRIAL TEST SESSION

Source	*df*	*MS*	*F*	*P*
Pretraining (P)	1	12.52	8.35	<.01
Age (A)	1	20.24	13.49	<.001
P × A	1	9.78	6.52	<.025
Within cells	50	1.50		
Total	53			

DISCUSSION

The results indicate clearly that the pretraining requiring *S*s to learn names for the discriminal stimuli resulted in a performance in the delayed reaction test that was superior to the performance of *S*s who had pretraining that involved only discrimination of the stimuli. The superiority of Group N appears due primarily to the younger *S*s, since the older *S*s in the two pretraining groups did not differ markedly. This interaction of the pretraining with age levels may be attributed to the fact that older *S*s of Group D were more inclined to invent names for the stimuli than were the younger *S*s. Six of the 8 *S*s in Group *D* who spontaneously verbalized names for the stimuli were in the older age level. On the other hand, the experimentally introduced names provided the younger *S*s of Group N with symbolic responses to the stimuli that they might not otherwise have attained.

There are two alternative, although not necessarily incompatible, explanations for these results. The first makes use of the hypothesis of acquired distinctiveness of cues (**3, 8, 9, 13**) which assumes that the learning of distinctive (verbal) responses to similar stimuli decreases the interstimulus generalization in subsequent learning of other discriminal responses. In the present experiment, the concentric circles were

made functionally more dissimilar for Group N by virtue of the distinctive names that had been learned for them. According to this explanation, Skalet's (**14**) animal forms may have been more distinctive than the geometric figures *because* the children had names available.

Another explanation of the findings is offered by Skalet's suggestion that the symbolic responses of articulate organisms serve as substitutes for the absent crucial stimuli (e.g., "box 'seven' is baited"). During the delay period, *S* reproduces the name for the baited stimulus, and following the delay, the name serves to direct his choice. This hypothesis is rather well supported by the verbalizations of *S*s in Group N during the test phase. Nine of the 27 *S*s spontaneously verbalized the name of the baited box during delay periods, sometimes repeating the name several times during a single delay period. This verbalization was not observed for *S*s in Group D. This hypothesis can also be used to account for the results of several experiments (**3, 4, 5, 7, 8, 9, 13**) in which was found that discrimination learning problems are learned more rapidly by *S*s who have had verbal pretraining with the relevant stimuli. In this case, the delay period that is presumed to be bridged by means of the names is the intertrial interval; that is to say, the names facilitate *S*'s recall of correct and incorrect stimulus-response sequences on previous trials.

Summary

Fifty-four preschool *S*s were divided into two groups. One group was given training on a pair of stimuli that involved learning distinctive names for the stimuli. The other group was given discrimination training with the stimuli without being required to learn their names. Subsequently, *S*s were given a delayed reaction test involving the pretraining stimuli. The *S*s who had learned names made more correct choices of the baited stimuli following delay than did *S*s who had not learned names, although this difference was due primarily to the younger *S*s. The results were interpreted in terms of a hypothesis that the possession of verbal names for the stimuli permits *S* to produce a representation of the absent stimuli during the delay period.

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INCIDENTAL LEARNING OF STIMULUS FREQUENCIES IN THE ESTABLISHMENT OF JUDGMENT SCALES [1]

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The judgment theories proposed by Helson (**2**), Johnson (**3**), and Volkmann (**6**) have been based in large part upon studies employing the method of single stimuli. This method requires *S* to judge each member in the presented stimulus series. While it imposes a procedural control necessary for the development of simple judgment principles, these principles should themselves be tested under more representative experimental conditions. The so-called "absolute" judgments of everyday life are not typically made by the method of single stimuli. A judgment of the aesthetic value of a painting, for example, may be made after exposure to many other paintings, none of which evoked an overt judgment. And yet we would assume that when such a judgment is made, it is influenced by preceding experience in ways similar to those in which the laboratory judgment is influenced by the preceding stimuli in the series being judged.

Experiments have shown that exposure to isolated, unjudged stimuli, interpolated between the judged members of a stimulus series, can have systematic effects upon the judgment scale (e.g., **1, 2**). In the present study, *Ss* are exposed to an extended series of unjudged stimuli under instructions to respond to some irrelevant characteristic of the stimuli. A comparison is made between the effects of this incidental exposure and those of the more traditional exposure conditions. These effects are studied in terms of *Ss'* subsequent judgments of a restricted range of stimuli and also in terms of other measures of learning.

The establishment or modification of a judgment scale involves learning in the sense that new judgments are given to old stimuli. What else is learned? The fact that judgment scales seem to depend upon the range (**6**) or distribution (**2, 3, 5**) of the stimulus series suggests that the learning of the stimulus distribution may be a crucial part of the judgment process. This hypothesis is supported by evidence that different sources of information about the stimulus distribution, verbal description (**4**) or special kinds of stimulus shift (**5**), cause *Ss* to judge the stimuli as if they had been drawn from the distribution suggested by the special information. The present experiment compares what *Ss* learn about stimulus frequencies under the usual judgment instructions with what they learn when exposed to the same stimuli but under instructions to respond to an irrelevant characteristic of the stimuli.

METHOD

Subjects.—Introductory psychology students, 300 in number, were assigned to 17 groups of from 8 to 29 *Ss* each. Assignments were made according to the times *Ss* were free to serve.

Materials.—The stimuli were square white cards, varying both in size and in the number of

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dots which were printed in the center of each card. The widths of the cards were as follows: 2, 2½, 3, 3½, 4½, 5½, 6½, 7½, and 9 in. The dots each had a diameter of ¼ in. The patterns in which they appeared formed a right triangle, 1¾ in. on both the vertical and horizontal sides, the right angle being closest to the lower-right corner of the card. The number of dots in the respective patterns was as follows: 3, 4, 5, 6, 7, 9, 12, 15, and 19. Each pattern appeared equally often on each of the different-sized cards so that there was no correlation between the two stimulus dimensions. For purposes of identification, the respective stimuli on both dimensions will be referred to by the numerals 1 through 9 (in order of increasing size or density).

Procedure.—Each group (the respective groups were run at different hours) was given response booklets in which to record the judgments and then read a set of judgment instructions, like those in (**5**), appropriate to the stimulus dimension on which they were to make their preshift judgments. The size-judging groups, I, III, VI, VIII, IX, and XII in Table 1, made their judgments in terms of a five-category size scale. The remaining groups were instructed to judge the numerousness of the dots, again in terms of a five-category scale. The stimuli were displayed manually to *S*s, being held one at a time, at 4-sec. intervals, before a black, 28 × 44-in. backdrop.

There were 88 preshift presentations for each of the 12 experimental groups. For three of the size-judging groups, VI, VIII, and XII in Table 1, each of the nine different sizes appeared with equal frequency (or approximately equal since two of the sizes appeared only nine times while the other seven were each shown 10 times). The other three size-judging groups, I, III, and IX, were given 16 presentations of each of the five largest cards and only two presentations of each of the four smallest cards. The dot-judging groups were similarly divided. In all cases, the different sizes and dot patterns were presented in quasi-random order and there was no correlation between the size of the card and the number of dots appearing upon it.

After Judgment 88, the groups to be tested for their recall of the stimulus frequencies (Groups I through VIII) were instructed to close their response booklets. Each *S* in these groups was then handed a page containing 12 frequency-estimation scales and told to record an estimate of the relative frequency with which each of the respective sizes (or patterns of dots) had been presented. Each group had to make these estimates in regard to the stimulus dimension on which they would make their post-shift judgments; this meant that half the groups, the incidentals, had to recall the stimulus frequencies in regard to the irrelevant dimension (the one which they had not been judging). For half of these groups, each frequency estimate was made in the presence of the stimulus whose past frequency was to be recalled; for the other half, no stimuli were shown during the frequency test, *S*s making their estimates in the order of recalled size (or numerousness of dots). In all cases, these estimates were checked on continuous scales running from "never" (0 mm.) through "less often than average" (37½ mm.), "average" (75 mm.), "more often than average" (112½ mm.) to "always" (150 mm.). Judgments of *S*s' confidence in these estimates were also checked on the same scales (see results for the measure used).

TABLE 1

SUMMARY OF EXPERIMENTAL CONDITIONS*

Group	Preshift Distribution	Frequency Estimates	Post-shift Distribution†
I	SIZE—skewed dots—rect.	SIZE—stimuli present	SIZE
II	DOTS—rect. size—skewed	SIZE—stimuli present	SIZE
III	SIZE—skewed dots—rect.	SIZE—stimuli absent	SIZE
VI	DOTS—rect. size—skewed	SIZE—stimuli absent	SIZE
V	DOTS—skewed size—rect.	DOTS—stimuli present	DOTS
VI	SIZE—rect. dots—skewed	DOTS—stimuli present	DOTS
VII	DOTS—skewed size—rect.	DOTS—stimuli absent	DOTS
VIII	SIZE—rect. dots—skewed	DOTS—stimuli absent	DOTS
IX	SIZE—skewed dots—rect.		SIZE
X	DOTS—rect. size—skewed		SIZE
XI	DOTS—skewed size—rect.		DOTS
XII	SIZE—rect. dots—skewed		DOTS

* Judged dimension in upper case.
† Both size and dot distributions rectangular; range of judged dimension: No. 1 to No. 5; unjudged dimension: No. 1 to No. 9.

After the frequency and confidence estimates had been made and the scales collected, all *Ss* were instructed to re-open their judgment booklets and to record their judgments of the remaining stimuli either in terms of the original size or dot categories (intentional groups) or in terms of the alternative set of categories (incidental groups). The distribution of the stimuli on the dimension being judged was shifted so that it now included only stimuli No. 1 through No. 5, the five smallest cards (or least-dense patterns). There were 110 of these postshift presentations, and each stimulus (in terms of the relevant dimension) appeared 22 times. The relevant stimulus values in both pre- and postshift series had the same distributions as those for the NR groups in (5) where the preshift series had provided an effective anchor for the postshift series. All nine of the irrelevant stimulus values (sizes or dot patterns) appeared with approximately equal frequency. Again, there was no correlation between the two stimulus dimensions.

The four remaining experimental groups, IX through XII, shifted directly to the postshift series without making the interpolated frequency estimates. These groups were also divided between intentional and incidental and between card-judging and dot-judging conditions. The experiment thus followed a $3 \times 2 \times 2$ factorial design. There were three conditions for the frequency and confidence estimates: (*a*) test with stimuli present, (*b*) test with stimuli absent, and (*c*) no test at all. A second variable, the judgment instructions, had two conditions: (*a*) incidental—frequency estimates and postshift judgments made in terms of the dimension which had been irrelevant during the preshift series, and (*b*) intentional—all judgments made in regard to the same stimulus dimension. The third variable, the nature of the stimuli, also had two conditions: (*a*) frequency estimates and postshift judgments made in regard to size, and (*b*) frequency estimates and postshift judgments made in regard to dots. All of the experimental conditions are summarized in Table 1.

Five additional groups served as controls. These first judged either the size or dot postshift series. They were then instructed to make frequency estimates in regard to either the judged or unjudged dimensions. Groups IX and X also made frequency estimates following the presentation of their postshift series. The specific control conditions will be described in greater detail when the comparisons between the experimental and control responses are made in the next section.

Results

Incidental learning of judgment scales.—The postshift judgments were converted to numerical form: very large (very many) = 5; large (many) = 4; medium (average) = 3; small (few) = 2; very small (very few) = 1. To simplify tabulation, only the judgments of the middle stimulus, No. 5, were used. Judgments of this stimulus showed the greatest change in the course of the postshift series. Two measures of each *S*'s postshift judgments were computed: (*a*) his initial postshift level, the mean of his first three postshift judgments of No. 5, and (*b*) his total postshift change, 3/10 of the sum of his last 10 postshift judgments of No. 5 minus the sum of his first three judgments of No. 5.

What effect did incidental vs. intentional exposure to the preshift series have upon the postshift judgments? As shown in Table 2, the incidental groups in each case judged stimulus No. 5 to be larger (or as having greater numerousness of dots) than it was judged by the corresponding intentional groups. They also showed, in five of the six comparisons, greater total change, i.e., greater adjustment to the postshift position of No. 5. This stimulus had been at the middle of the preshift range, but it was the largest (or the one with the most dots) in the postshift series. Thus, the lower judgments by the intentional groups reflected a greater anchoring effect by the intentionally learned preshift series. The statistical significance of this effect was indicated by analysis of variance (following the procedure described for unequal *N*'s in 7), the effects of incidental vs. intentional preshift exposure being significant ($P < .01$) in both the initial-level and total postshift change analyses (total-change scores logarithmically transformed to reduce heterogeneity of variance to $P > .10$).

Additional information concerning

TABLE 2

MEAN POSTSHIFT JUDGMENTS

Stimulus	Intentional						Incidental					
	Initial Level				Total Change*		Initial Level				Total Change*	
	Group	*N*	Mean	*SD*	Mean	*SD*	Group	*N*	Mean	*SD*	Mean	*SD*
Frequency Test with Stimuli Present												
Size	I	13	2.61	.66	.768	.152	II	14	3.17	.61	.770	.111
Dots	V	29	2.82	.46	.731	.065	VI	27	3.21	.52	.833	.126
Frequency Test with Stimuli Absent												
Size	III	15	2.95	.44	.775	.106	IV	15	3.91	.54	.775	.126
Dots	VII	24	2.89	.51	.801	.166	VIII	27	3.73	.68	.875	.098
No Frequency Test												
Size	IX	18	2.33	.42	.733	.126	X	11	3.79	.48	.849	.089
Dots	XI	24	2.53	.48	.742	.100	XII	17	3.16	.73	.906	.140

* Each score = log (total postshift change + 5.0).

the effect of the preshift experience upon the postshift judgment scales was obtained through comparison with control groups whose *first* judgments were either of the postshift size or postshift dot series. In both cases, these control means were significantly higher ($P < .001$) than the means of the corresponding intentional groups not receiving an interpolated frequency test (Groups IX and XI). The same comparison for the corresponding incidental groups (X and XII) yielded a statistically significant t for the dots ($P < .01$) but not for the sizes ($P > .40$); though here too the difference was in the correct direction. There is thus evidence for the incidental learning of judgment scales, but such learning appears to be less effective than intentional learning.

Incidental learning of stimulus frequencies.—Since the larger stimuli (or the ones with more dots, depending upon which dimension would later be tested) were presented more frequently during the preshift series, learning of the stimulus frequencies would be indicated by greater frequency estimates for these stimuli. For each *S*, a frequency-estimate score was obtained by subtracting the sum of his estimates (distance in millimeters between his frequency check and the "never" mark on each scale) for stimuli No. 1 through No. 4 from the sum of his estimates of stimuli No. 6 through No. 9 (comparable measures for *S*s tested in the absence of the stimuli being based on the differences between the sums of the estimates for the first and last half of the scales, adjusted in each case for the total number of scales used).

What were the differences in frequency learning as a result of inci-

dental vs. intentional exposure to the stimulus series? For each of the four appropriate comparisons, the intentional group had a higher frequency-estimate score than the corresponding incidental group (see Table 3). This greater accuracy in discrimination of the skewness of the preshift series was statistically significant ($P < .01$ in an analysis of variance performed upon these scores).

Was there any incidental learning of stimulus frequencies? Comparison with control groups tested following incidental exposure to the rectangularly distributed series (sizes or dots) indicated that when the frequency test was made *in the presence* of the respective stimuli, even the incidental groups showed significant discrimination of skewness of the series ($P < .01$). However, the evidence for such discrimination fell short of customary statistical standards when the frequency estimates were made in the absence of the stimuli ($P > .05$). This dependence of the results upon the method of test was reflected in the analysis of variance of the experimental groups' frequency-estimate scores by significant mean squares associated both with the method of test and with the interaction between the test method and the nature of preshift exposure ($P < .05$ and .01, respectively).

Incidental learning of stimulus range.—Since stimulus range may be a major determinant of judgment (7), stimuli one step below and one step above the regular series were also displayed during the frequency-estimate test for Groups I, II, V, and VI. These stimuli had not previously been shown, and therefore smaller frequency estimates for them would indicate more accurate discrimination of the stimulus range. An analysis of variance performed upon these estimates indicated that the intentional learning was again superior to incidental learning ($P < .01$); but even for the incidental groups, the mean estimate of these stimuli was significantly better ($P < .001$) than chance (which would have been a

TABLE 3

MEAN FREQUENCY ESTIMATES AND CONFIDENCE RATINGS

Stimulus	Intentional							Incidental						
	Group	Frequency Estimates			Confidence Ratings			Group	Frequency Estimates			Confidence Ratings		
		N	Mean	*SD*	*N*	Mean	*SD*		*N*	Mean	*SD*	*N*	Mean	*SD*
Frequency Test with Stimuli Present														
Size	I	13	168.5	80.0	12	34.7	14.3	II	16	107.4	81.5	13	46.8	15.0
Dots	V	25	231.1	71.3	24	38.1	9.3	VI	24	134.3	82.5	25	56.2	14.6
Frequency Test with Stimuli Absent														
Size	III	14	58.0	82.3	14	44.6	14.8	IV	12	46.6	105.6	12	49.9	14.6
Dots	VII	20	107.1	82.5	21	51.4	8.7	VIII	27	90.4	88.6	28	54.8	13.0

check at "with average frequency," the mid-point of the frequency-estimate scale).

Confidence estimates.—For each frequency estimate, *S* was asked to indicate with two vertical lines the range of relative frequencies he would accept as possible for that stimulus. Thus, the shorter the distance between lines, the greater *S*'s confidence in his frequency estimates. Mean confidence ratings were computed for each *S*. As shown in Table 3, each intentional group was more confident than the corresponding incidental group, the difference exceeding the .01 level in an over-all analysis of variance.

Discussion

The evidence that a judgment scale may be established through incidental exposure to a series of stimuli, while perhaps not surprising, does suggest a certain generality for principles of judgment which have been based upon studies using the traditional method of single stimuli. However, incidental exposure has less of an effect upon the judgment scale than exposure under instructions to judge. This finding extends the results obtained by Helson (**2**) and Brown (**1**) using interpolated, unjudged stimuli without distracting cover tasks. It thus appears that mere stimulus exposure, without any set or instructions in regard to the stimulus dimension that will subsequently be relevant, has demonstrable effects upon judgment.

The results also suggest that speculation about judgmental processes can be bolstered with independent measures obtained in the judgment situation. They show that *S*s acquire knowledge of both the range and the relative frequencies of stimuli to which they are exposed. Such knowledge is more accurate when *S*s have been instructed to make absolute judgments of the stimuli than when their attention has been directed to an irrelevant stimulus dimension, and they know that it is. This greater confidence in what they have learned is paralleled by greater resistance to the demands of a changed stimulus series.

This last finding, while predictable on a common-sense basis, would seem to offer some difficulty for a general quantitative theory of judgment. Simple differential weighting (**2**, p. 5; or **3**, p. 349) could handle the greater initial anchoring effect of the judged preshift series, but a much more complex formulation would seem necessary to describe the greater postshift change by the incidentally exposed groups. Nor is this finding predictable from the general hypothesis developed in (**5**)—that postshift change will be greater when the postshift series is a less likely sample from the preshift distribution; for here the respective stimulus distributions are the same for all groups.

One might speculate that the incidentally exposed *S*s, having less confidence in what they had learned about the preshift stimuli, quickly forget during the postshift series that the missing stimuli have ever been shown. Frequency estimates obtained *after* the postshift series would provide a test for such speculation. However, the fact that there was little evidence for frequency learning when the test was made in the absence of the stimuli suggests limitations for both *E*'s speculations and *S*'s retrospections. Hypotheses, strategies, or expectancies appear much more elusive than common sense would have them—existing, perhaps, only as extremely limited response potentialities.

Summary

This experiment studied the effects of incidental exposure to an extended series of stimuli. An attempt was made to assess what *S*s learned about the stimuli both in terms of their estimates of the respective stimulus frequencies and also in terms of their absolute judgments of subsequently exposed stimuli. These indexes of learning were compared with those for *S*s exposed to the same series, but under instructions

to judge each stimulus in terms of the subsequently relevant stimulus dimension.

The results indicated that judgment scales may be incidentally learned but that such learning is less effective and also more subject to interference. Instructions to judge the stimuli facilitated learning of both their range and relative frequencies, and also increased *Ss'* confidence in their own frequency estimates. These results are consistent with the assumption that establishment of an absolute judgment scale involves the acquisition of information about the distribution of the stimuli presented for judgment.

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THE INFLUENCE OF MEANINGFULNESS, INTRALIST SIMILARITY, AND SERIAL POSITION ON RETENTION

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The first purpose of the present study is to determine the influence of meaningfulness on recall of rote-learned materials. While no adequate study has been published dealing with this variable, it is a common belief (e.g., **5, 6, 10, 16**) that the more meaningful the material the better the retention. Only Osgood (**8,** p. 562) has questioned the assumption. Recent findings on recall as a function of ability level (**13**) and intralist similarity (e.g., **11**) suggest that variables which influence rate of learning may have little or no influence on recall. Our first objective, then, is to test recall as a function of meaningfulness when response strength of items of different levels of meaningfulness have been equated at the end of learning.

As indicated above, our previous studies have suggested that intralist similarity is a minor variable at recall. In general we have found no difference in recall as a function of intralist similarity although in two cases (**12, 15**) the recall was better for the high-similarity lists. But, as pointed out elsewhere (**14**), these determinations may have been confounded by the reciprocal relation between intralist and interlist similarity when *Ss* serves in a series of conditions as they did in our experiments. Therefore, we felt it advisable to make at least one further test of the similarity variable using *Ss* who had had no previous formal experience with the materials. This rules out interlist similarity as a source of confounding but does not, of course, eliminate extra-experimental learning as a source. In any event, we feel that the present test of the influence of intralist similarity on retention is as "clean" as it is possible to make it using standard verbal materials.

Finally, we wanted to determine the influence of serial position on recall. Evidence from Hovland (**4**) indicates that on recall 24 hr. following learning, items at each serial position are recalled about equally well, but that on the immediately succeeding trial the classical bowed curve is quite evident. However, Hovland's findings at recall may be heavily influenced by proactive inhibition from the many previous lists learned by his *Ss*. Furthermore, only if items at different serial positions are equated for response strength at the end of learning can the effect of serial position on recall be evaluated unconfounded by differences in response strength which probably are present for items at different serial positions at the end of learning.

METHOD

Materials.—Four serial lists of nonsense syllables of 10 items each were used. Two of the lists were made up of items of from 93.3% to 100% association values and the other two from among items of from 0.0% to 20.0% association value (**2**). One of the two lists at each level of meaningfulness had high intralist similarity, one low intralist similarity. The two lists with high intralist similarity had only four consonants,

[1] This work was done under Contract N7onr-45008, Project NR 154-057 between Northwestern University and the Office of Naval Research.

TABLE 1

LISTS USED

High Meaningfulness		Low Meaningfulness	
High Similarity	Low Similarity	High Similarity	Low Similarity
(HM-HS)	(HM-LS)	(LM-HS)	(LM-LS)
BES	DOZ	GOK	CEF
RUL	SOC	GUW	DAX
SAR	QIL	GYK	GAH
BIL	MEK	KIG	MEQ
ROS	FEV	KEZ	SIJ
LIB	HIN	WUG	TOV
SEL	PUR	WEZ	NUB
BOR	YUT	ZIK	YIL
LIS	GAB	ZYW	ZOK
RAB	WAK	ZOW	WUP

each being used five times. In the lists with low intralist similarity each consonant was used only once. In each list each vowel was used twice. The four lists are given in Table 1. Letting H stand for High, L for Low, S for Intralist Similarity, and M for Meaningfulness, the four lists may be symbolized as follows: HM-LS, HM-HS, LM-LS, LM-HS.

Ten orders of items for each list were prepared such that each syllable occurred equally often in each serial position. Each order was used an equal number of times so that bias due to a particular syllable being in a particular serial position should be minimal. The list was always preceded by three asterisks as the anticipatory cue for the first syllable.

Subjects and procedure.—The four different lists represent the four conditions of the experiment. A total of 400 *S*s served in the experiment, 100 for each list. The use of this rather large number of *S*s is necessary for the type of analyses we wish to make and does not indicate a repudiation of small-sampling procedure. On *E*'s schedule sheet the order of the four lists was random with the restriction that each occurred 100 times. The *S*s were then assigned in order of appearance at the laboratory. Ten *S*s were lost for failure to learn the most difficult list (LM-HS) and one for failure to learn the LM-LS list. None was lost for failure to learn the other two lists. Therefore, there is some selection of *S*s in that the 100 *S*s completing the LM-HS list probably were of higher average rote-learning ability than were those learning the other lists. Consequently, differences in rate of learning among lists are probably reduced when comparisons are made between LM-HS and any of the other three lists. This selection, however, has no effect on the retention data, as will subsequently be shown.

All *S*s were naive to the formal rote-learning situation. Each *S* learned the list to which he was assigned to one perfect trial. Recall and relearning to one perfect trial took place 24 hr. after learning. Lists were presented at a 2-sec. rate on a Hull-type drum with 8 sec. between trials. All syllables were spelled.

RESULTS

Learning.—The essential learning data are shown in Table 2. In terms of mean or median number of trials to learn, the effects of the two variables are evident and confirm a number of previous studies; the higher the meaningfulness the faster the learning, and the lower the intralist similarity the faster the learning. Because of heterogeneity of variance which was not corrected by transformations, we have used the median test (7, p. 402). Both intralist similarity and meaningfulness are significant beyond the 1% significance level and the Similarity × Meaningfulness interaction is significant between the 5% and 1% levels. The interaction indicates that the lower the meaningfulness the more effective is intralist similarity in retarding learning.

The analysis of variance on mean errors per trial shows that meaningfulness does not influence the number of errors made. Similarity, on the other hand, gives an F of 5.03 ($.05 > P > .01$). However, as can be seen from Table 2, this difference is completely produced by the lists of

TABLE 2

TRIALS TO LEARN AND ERRORS PER TRIAL DURING LEARNING

List	Trials			Errors	
	Mean	σ_M	Median	Mean	σ_M
HM-HS	25.86	1.18	24.5	1.47	.07
HM-LS	19.27	.92	17.3	1.47	.07
LM-HS	42.57	1.72	40.8	1.61	.07
LM-LS	29.18	1.54	27.5	1.32	.06

TABLE 3

NUMBER CORRECT AT RECALL AND NUMBER OF TRIALS TO RELEARN

List	Recall		Relearning	
	Mean	σ_M	Mean	σ_M
HM-HS	6.96	.21	4.89	.38
HM-LS	6.90	.21	3.71	.20
LM-HS	6.33	.20	7.59	.63
LM-LS	7.10	.20	5.23	.41

low meaningfulness, where the greatest number of errors occur with high intralist similarity. This is supported by an F of 5.00 for the Meaningfulness × Similarity interaction. In a previous study (**11**) using syllables in the middle range of association value, no difference in error frequency as a function of intralist similarity was found in spite of large differences in rate of learning. It appears, therefore, that unless the material has low meaningfulness, no difference in error frequency as a function of intralist similarity may be expected in serial lists. Also, it may be worth noting that the relative error frequency among conditions during learning and the relative recall among conditions (Table 3) correspond closely.

Although meaningfulness does not influence error frequency, it is possible that the distribution of the errors in the course of learning could be different. However, when Vincent-type curves were plotted for each list no appreciable differences among the curves were apparent. In all four cases the error frequency increased somewhat during the first third of learning, remained about constant for the second third, and decreased for the last third.

Recall.—The raw recall and relearning scores are shown in Table 3. Neither meaningfulness nor intralist similarity significantly influences recall. Analysis of variance shows that the Meaningfulness × Similarity interaction just reaches the 5% level. This indicates that with low meaningfulness intralist similarity influences recall whereas with high meaningfulness it does not. The recall is less for high intralist similarity than for low.

Since wide differences in rate of learning of the different lists were evident, analysis of the raw recall scores is inappropriate as a means of getting the best possible estimate of the influence of the variables (**13**). Our major analyses, therefore, are based on probability analyses of acquisition curves as a means of adjusting for different response strengths at the end of learning so that recall can be evaluated without bias due to different initial strengths for the items in the different lists. Before turning to these analyses, however, we must dispose of the problem of different ability level of the *S*s learning the lists. As indicated previously, *S*s learning the LM-HS List probably had higher ability level than those learning the other three lists. While we have shown elsewhere that ability level is not related to retention (**13**), we have made corresponding analyses for the 100 *S*s who learned each list in the present study. For each group of 100 *S*s the median of the learning scores was found and then the 50 rapid-learning *S*s were compared on recall with the 50 slow-learning *S*s. In no case was there a difference in recall when response strengths at end of learning were equalized. Therefore, the influence of the major variables on recall can be determined without bias due to differences in ability level of the *S*s in the four groups.

In determining the amount lost over the 24-hr. retention interval, we first constructed successive probability curves for the learning of each list. For each *S* for each list an estimate was made of the number of correct responses he would have gotten had there been another learning trial. This estimate was based on the successive probability curve for all *S*s in the group. Then for each *S* the actual number of items correct on recall was subtracted from the number expected to give a loss

TABLE 4

MEAN LOSS SCORES

	List			
	HM-HS	HM-LS	LM-HS	LM-LS
Mean	1.41	1.48	1.90	.96
σ_M	.20	.22	.20	.19

score. Details and rationale of this procedure have been presented elsewhere (**9**). Table 4 gives the mean loss score for each of the four conditions.

We note first that, with high meaningfulness, intralist similarity has little influence on forgetting, but that with low meaningfulness the loss is greater for the list of high intralist similarity than it is for the list of low intralist similarity. This difference, however, is a function of two changes neither of which is statistically significant separately. First, with high similarity there is an increase in the loss from high to low meaningfulness. Second, with low similarity, there is a decrease in the loss from high to low meaningfulness. The smallest absolute loss occurs with the list having low meaningfulness and low intralist similarity. By analysis of variance the effects of Similarity and Similarity × Meaningfulness are both significant beyond the 5% level of confidence.

As indicated above, there is a tendency for the list of low meaningfulness to be better recalled than the list of high meaningfulness when intralist similarity is low. However, since this does not attain statistical significance, we must conclude that for the materials used here meaningfulness does not influence retention. Since this conclusion is somewhat contrary to the usual assumption, we present in Fig. 1 the recall as a function of strength of items for the 200 *Ss* learning the high-meaningful lists and the 200 *Ss* learning the low-meaningful lists. The percentage of items recalled is plotted against base lines which have been adjusted so that the strength of items at end of learning for the two levels of meaningfulness are equal at any given point along the base lines (**13**). If there is any trend discernible, it is that the list of low meaningfulness is better recalled than the list of high meaningfulness.

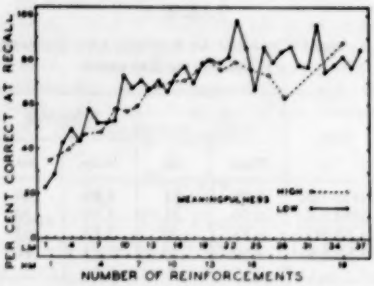

FIG. 1. Recall as a function of meaningfulness when strength of associations have been equalized at end of learning.

We turn next to an analysis of recall as a function of serial position. In Fig. 2 the mean number of correct responses at each serial position has been plotted. Contrary to Hovland's results (**4**), there is a bowed curve quite comparable to that obtaining during acquisition. More important

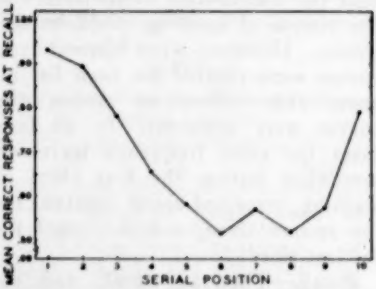

FIG. 2. Mean number of items recalled at each serial position for all lists combined.

for our purposes, however, is to determine whether the differences in recall as a function of serial position are largely a function of differences in strength of items at end of learning or whether something intrinsic to serial position accelerates forgetting. To do this we have again used the probability analysis of learning and then have adjusted base lines for recall so that expected probabilities of immediate recall are the same for all serial positions. To add some stability to the data we have combined Serial Positions 1, 2, and 3; 5, 6, and 7; and 8, 9, and 10, omitting Position 4 entirely. We are thus dealing with three sections of the bowed curve. For each section independent probability analyses of learning were made. From these, base lines were adjusted so that the strength of items at end of learning for each section was equivalent. The percentage correct on recall for each section was then plotted along the adjusted base lines as shown in Fig. 3. Although there is great irregularity, there is no evidence that the first and last sections of the list are any better recalled than the middle section. There is a suggestion that the middle items (5, 6, and 7) are better recalled than the end items when such items are weak. However,

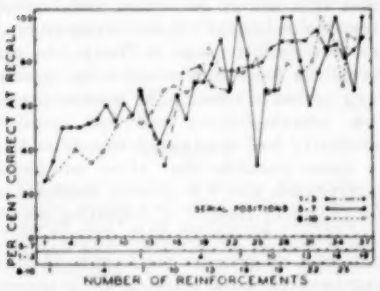

FIG. 3. Recall as a function of serial position when strength of associations have been equalized at end of learning.

it would appear that the most conservative conclusion is that there is no difference in retention as a function of serial position when strength of items at end of learning is equalized.

Relearning.—The mean number of trials required to relearn each list was given in Table 3. These values reflect almost perfectly the learning values as given in Table 2. By analysis of variance both meaningfulness and intralist similarity are highly significant sources of variance in relearning. Contrary to learning scores, however, the interaction between meaningfulness and similarity does not even approach significance.

DISCUSSION

The discussion will be built around the implications of the present results for an interference theory of forgetting. We may first note that the absolute amount of forgetting in the present experiment is much less than usually reported. For example, in Hovland's study (**4**) in which 12 syllables were learned and recalled after 24 hr., the mean number recalled was 2.61. In our previous study (**11**) using 13 syllables the mean number recalled was approximately 4.5, varying in small amounts for different conditions. In both experiments *S* served in a number of conditions. In the present study the average number recalled for all conditions was 6.82 out of the 10-item list. Such findings make it quite clear that when *S* serves in a series of conditions the major source of interference is the previous lists learned (**3, 14**). In the present study no previous lists had been learned to interfere with recall attempts. And, when we measure forgetting with expected immediate recall as a base, the amount of forgetting ranges from .96 to 1.90 items. It is apparent that compared with usual findings not much forgetting has taken place, and that when previously learned lists are eliminated as a source of interference, the amount of forgetting for

which an interference theory must account is markedly reduced. It should also be mentioned that there is little evidence of lack of warm-up. For all lists combined only 1.3% forgetting was observed in the first item in the list and 1.2% in the second. The first item may, therefore, be depressed a very small amount due to inadequate set, but it is certainly of little consequence for the over-all retention of the list.

Osgood (**8**) has indicated that to maintain an interference theory it must be shown that low-meaningful material is better retained than high-meaningful material. This must be so because verbal habits learned in everyday experiences would not as likely interfere with low-association syllables as they would with high-association syllables. In the present study we did not find statistically significant differences in retention between the high- and low-meaningful syllables, but the differences were in the expected direction. More extreme levels of meaningfulness (such as meaningful words *vs.* consonant syllables) might well show larger differences. In any event, it is quite clear that with intralist similarity low, serial syllables of high meaningfulness are not better recalled than syllables of low meaningfulness.

Gibson's theory (**1**) holds that the major source of forgetting is the interference produced by recovery of intralist generalization tendencies with the passage of time. As indicated elsewhere (**14**) we have found this a very fruitful notion provided that the interference produced by recovery of generalization is extended to include previous lists and previous extra-experimental learning. Earlier attempts to support the theory were clouded by the confounding of interlist and intralist similarity. In the present experiment interlist similarity was eliminated. So far as we know, the difference in recall for high and low intralist similarity for the low-meaningful lists is the first clear-cut substantiation of the Gibson hypothesis. According to the theory there is recovery of generalization among similar items over the 24-hr. interval introducing more interference at recall than in the case of a list with low similarity. But why doesn't the same difference obtain for the lists of high meaningfulness? Two possibilities seem most likely. First as implied in Gibson's theory, the more meaningful the material the less rapid the spontaneous recovery of generalization. This would mean that for longer retention intervals than used here we would predict that sooner or later the high-similarity list would show greater forgetting than the low-similarity list. The second possibility is that with high intralist similarity there is less interference from previous learning. An experiment in which high-similarity lists were ***better*** retained than low-similarity lists suggest this possibility (**15**). This position would say that for the lists of high meaningfulness, high intralist similarity did produce a decrement in retention due to interference from within the list but a roughly equivalent amount occurred in the low-similarity list because of interference from previous learning. We have no basis in the present data for choosing between these two alternatives.

The above considerations lead to certain definite principles. If a single list is learned, the list with the lowest meaningfulness and the highest intralist similarity (within limits) will show most rapid forgetting. The list with the lowest meaningfulness and lowest intralist similarity will show the slowest rate of forgetting. Indeed, it seems possible that if a list of consonant syllables of very low meaningfulness was constructed so that no letter was duplicated in the list, little forgetting would occur over a long period of time. The present list of low meaningfulness and low intralist similarity had duplicated vowels and it is quite possible that these produced interference which is entirely responsible for the small amount of forgetting which occurred. As meaningfulness increases, however, we increase the likelihood that extra-experimental learning will interfere with recall. We cannot be entirely confident that relative magnitude of forgetting suggested by these principles

will hold with paired-associate learning, since previous analyses (**14**) suggest that paired-associate lists are more susceptible to interference from previous habits than are serial lists. Even with very low-meaningful lists of paired associates it is possible that forgetting would be appreciable over 24 hr.

Hovland (**4**) found little difference in recall as a function of serial position after 24 hr., whereas the present recall results show the typical bowed curve as found in learning. It is our belief that this contradiction is related to differences in interlist interference between the two experiments. Hovland's *S*s had learned many serial lists which would interfere considerably with recall. Not only would there be interference among specific items but also among lists as wholes, since the same anticipatory cue for the first syllable was the same for all lists. The interference should be greatest during the attempted recall of the first several items as *S* would have difficulty in establishing "which list this was." Thus, recall of the first items should be severely depressed. By the time the middle items appeared, however, *S* should have established what list was involved so that these items could be as well recalled as the first items even though they are weaker. Yet by this reasoning the items in the last portion of the list should be better recalled than any; this is not the case, and at the present time we have no resolution of this difficulty. Nevertheless, the fact that in Hovland's study the bowed curve was very apparent on the second relearning trial is quite in line with previous findings of the transitoriness of interference among serial lists (**14**).

Summary

The purpose of this experiment was to determine the influence of meaningfulness, intralist similarity, and serial position on retention of serial lists when *S*s had had no previous laboratory experience with such lists. Nonsense-syllable lists of 10 items each were used, the four lists representing two extremes of meaningfulness and two extremes of intralist similarity. A total of 400 *S*s, 100 for each list, learned to one perfect trial and recalled and relearned after 24 hr. The results show:

1. In learning, the lower the intralist similarity the faster the learning and the higher the meaningfulness the faster the learning. Intralist similarity produced a greater difference with the low-meaningful lists than with the high.

2. For low meaningfulness, recall was significantly better with low intralist similarity than with high intralist similarity. No difference was observed for the high-meaningful lists.

3. Meaningfulness did not significantly influence recall.

4. Recall was not related to serial position when response strengths at end of learning were equalized for all serial positions.

The amount of forgetting observed was considerably less than found with comparable materials in experiments in which *S* served in several conditions. The difference seems almost without doubt to be due to large amounts of interlist interference which occurs when *S* serves in several conditions. The results provide strong support to a general theory of interference as a prime cause of forgetting and to Gibson's theory in particular.

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TEMPO OF FREQUENCY CHANGE AS A CUE FOR DISTINGUISHING CLASSES OF SPEECH SOUNDS[1]

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In recent experiments with synthetic speech (**3, 5, 6, 7**) we have isolated many of the acoustic cues by which a listener identifies various consonants of American English. Some of the cues that we have found to be important for the voiced stops (*b, d, g*), the voiceless stops (*p, t, k*), and the nasal consonants (*m, n, ŋ*) are illustrated in the hand-painted spectrograms of Fig. 1. These spectrographic patterns are very highly simplified and schematized by comparison with spectrograms of real speech, yet capable of producing fair approximations to the intended consonant-vowel syllables when converted into sound by an appropriate playback instrument. The vowel, which is *a* in all cases, is given by the two concentrations of acoustic energy, or "formants" as they are called, the one centering at about 720 cps and the other at 1320 cps. The consonant cues, with which we will be more concerned, begin at the left-hand side of each pattern and extend to the point at which the formants assume the steady state that characterizes the vowel.

As can be seen in the hand-painted spectrograms, the acoustic cues that distinguish the columns of the figure (i.e., *b–p–m* from *d–t–n* from *g–k–ŋ*) are the direction and extent of the relatively rapid shifts in the frequency position of the second (higher) formant. These frequency shifts, or "transitions," are typically found in spectrograms of real speech at the junction of consonant and vowel.[4] By converting patterns like those of Fig. 1 into sound, we have found that the second-formant transitions can, in fact, be cues for the perceived distinctions among the three classes, *b–p–m*, *d–t–n*, and *g–k–ŋ*. There are other acoustic cues for these same classes—we have discovered that appropriate transitions of the third formant, for example, contribute to the identification of these sounds—but our results indicate that the second-formant transitions are very nearly sufficient.

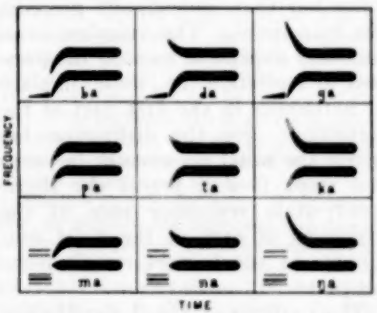

FIG. 1. Hand-painted spectrograms of nine consonant-vowel syllables, showing some of the principal acoustic cues for the perception of the stop and nasal consonants.

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[2] Also at the University of Connecticut.

[3] Also at the University of Colorado.

[4] The transitions presumably reflect the changes in the oral cavities that necessarily occur as the articulators move from the position of the consonant to that of the vowel.

We know rather less about the cues that distinguish the patterns that form the rows (i.e., *b–d–g*, *p–t–k*, *m–n–ŋ*). Though we have isolated several cues for the distinction between the voiced (*b–d–g*) and voiceless (*p–t–k*) stops, we have not yet investigated the problem carefully enough to know which of these are most important. That omission need not concern us here, however, since for this paper the most relevant consideration is the evidence that each of these cues is essentially constant within a row (of Fig. 1) and more or less capable of distinguishing that row from others. As shown in the figure, one cue that marks the voiced stops (*b–d–g*) as a class is the low-frequency voice bar that immediately precedes the transitions. The voiceless stops probably depend in part on the presence of aspiration (i.e., noise) in place of harmonics in the first part of the transition. For the distinction between the nasal consonants (*m–n–ŋ*) and stops (*b–d–g*, *p–t–k*) the short, steady-state resonance seen at the beginning of each of the nasal consonants in the bottom row is quite adequate.

The patterns of Fig. 1 provide the basis for a table of acoustic elements out of which many of the sounds of speech can be made.[5] The table is interesting, we think, because it indicates that a small number of cues on one acoustic dimension (second-formant transition) combine in all possible pairs with various acoustic markers (the constant resonance of the nasal consonants, for example) to produce some of the highly distinctive sounds of speech. Thus, highly identifiable stimuli are created out of the wholly unpatterned combination of simple and discrete elements. We expect that the table of Fig. 1 will ultimately be expanded to include all the consonant sounds. It is one of the purposes of this study to take a step in that direction.[6]

Exploratory work suggested that we might convert the patterns of Fig. 1 into different speech sounds by varying only the tempo of the transitions, thus pointing to time as another of the stimulus dimensions which may be important in the perception of the individual phones of speech. As the transitions are progressively slowed, the pattern for the stop consonant *b* plus the vowel *a* (as in *bottle*) begins to be heard as the semivowel *w* plus *a* (as in *wobble*), and then as a vowel

[5] The 3 × 3 table of Fig. 1, in which the patterns are arranged in terms of the acoustic cues, parallels a commonly accepted phonetic classification according to the articulatory dimensions of *place* and *manner* of articulation. The three columns correspond to three places of production (i.e., points along the oral tract at which the consonant closure is made), *p–b–m* being produced at the lips, *t–d–n* at the alveolar ridge, and *k–g–ŋ* at the velum. Manner of production refers to the particular articulatory features (for example, the presence or absence of voicing or nasality) that are common to the sounds in any given row.

[6] Students of language have long found it useful to describe each consonant as an articulatory event in which one of a small number of places of production is combined with one manner. The specification of place and manner is, then, sufficient to describe a consonant uniquely. To the extent that the relation between articulation and sound is not too complex, we should expect that the categories of place and manner might be equally appropriate for a classification of the acoustic characteristics. That such a classification has utility for a study of speech perception has recently been shown in an important experiment by Miller and Nicely (8). They took account of the confusions that occur when English consonants are heard against progressively increasing amounts of noise and found, for example, that a listener will continue to identify a sound correctly as being a member of a certain manner class even though he does not hear it accurately in regard to its place of production. In general, their results indicated that the place and manner cues, whatever their nature, may be perceived quite independently of each other.

which changes color from *u* to *a* (as in *too odd*). Similarly *ga* (as in *goggle*) goes to *ja* (as in *yacht*), and then to *ia* (as in *theology*). The remaining stop, *d*, goes through comparable transformations, producing (with the vowel *a*) the semivowel *ɥ* (as in French *nuage*) and then the vowel-of-changing-color *ya* (as in French *cru a*).[7]

The experiments to be reported here were designed to extend our exploratory observations concerning transition tempo as a cue for the perceived distinctions among stop consonants, semivowels, and vowels of changing color.

Experiment I

In this part of the study we have tried to determine whether changing the tempo of first- and second-formant transitions is sufficient to convert the syllables *bɛ* and *gɛ* (stop consonant plus vowel) into *wɛ* and *jɛ* (semivowel plus vowel) and, in the extreme, into *uɛ* and *iɛ* (vowels of changing color). The semivowel (plus vowel) *ɥɛ* and vowel-of-changing-color *yɛ*, which presumably result from a slowing of the *d* transition, are not familiar to our American listeners and were for that reason excluded from the experiment.

The vowels of changing color are very different linguistically (and perceptually) from stops and semivowels in that they are not single linguistic units and occur only in situations in which one of the vowels belongs to one syllable and the other to the next. We thought it appropriate, therefore, to deal separately with the two-category distinction between stop and semivowel and the three-category choice among stop, semivowel, and vowel of changing color.

Method

General aspects of procedure and apparatus.—In these experiments hand-painted spectrograms have been used as a basis for creating and controlling speech-like sounds. To convert the spectrograms into sound—an obviously necessary step in this method—we take advantage of a special-purpose instrument called a pattern playback. Descriptions of this instrument, together with discussions of our method, are to be found in earlier papers (**1, 2, 4**).

The playback employs a variable-density tone wheel to modulate the light from a mercury arc, producing a fundamental of 120 cps and all its harmonics through the fiftieth at 6000 cps. The modulated light beams are imaged on the spectrogram, and are so spread across it as to match its frequency scale. As the hand-painted spectrogram is moved through the light, the white paint reflects beams whose modulation frequencies correspond to the position of the paint on the frequency scale of the spectrogram. The reflected beams are led by plastic light guides to a phototube, the current of which is amplified and converted to sound.

Stimuli.—To vary the tempo of the transitions, we prepared (for conversion into sound) spectrographic patterns like those shown in Fig. 2. The series of patterns illustrated in the top and bottom rows were designed to produce *bɛ*, *wɛ*, *uɛ*, and *gɛ*, *jɛ*, *iɛ*, respectively. In each series, the duration of first- and second-formant transitions was varied from 10 msec. to 300 msec. in steps of 10 msec. When we vary the duration of the transition, we are also, of course, varying the rate of transition (that is to say, the frequency shift per unit time) so long as the frequency extent of the transition remains constant. For convenience, we will present specific values in terms of duration; however, we will continue to speak of this variable generally as tempo.

As can be seen in Fig. 2 the first- and second-formant transitions were varied together, the durations of the two being always equal in any one sound. This approximates what would seem, on the basis of an inspection of spectrograms, to happen in real speech.

The steady-state portion of the pattern has a duration of 300 msec. in all cases, and its formants are so placed as to produce an approximation to the vowel *ɛ*. By using this vowel, it

[7] In articulatory terms *b* and *w* have a common place of production at the lips; *d* and *ɥ* are both produced at the alveols. The semivowel *j* is normally articulated at a point slightly forward of the velar *g*; however, there is in English no semivowel whose place of articulation is closer to *g*.

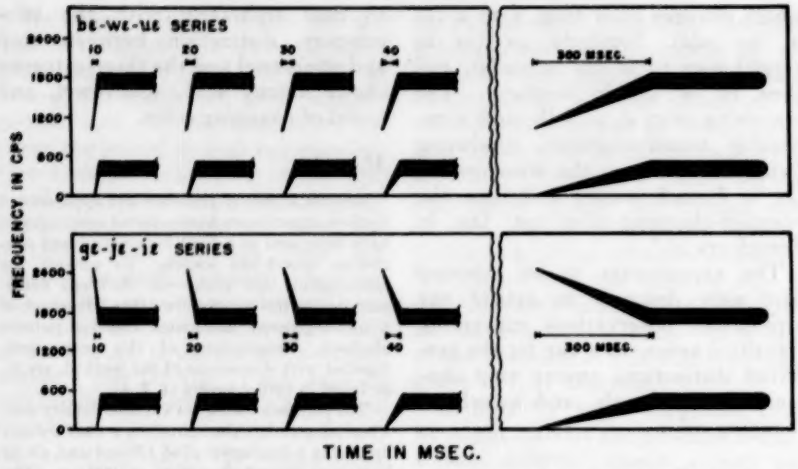

FIG. 2. Illustrations of the spectrographic patterns used to produce the stimuli of Exp. I. The first four patterns in each row show how the tempo of the transitions was varied. At the extreme right of each row is a complete stimulus pattern, i.e., transition plus steady-state vowel, for the longest duration of transition tested.

has been possible to make the frequency shifts for the *bɛ–wɛ–uɛ* series exactly equal in extent to those that produce *gɛ–jɛ–iɛ*. The *b* transition begins 720 cps below, and the *g* transition 720 cps above, the steady state of the second formant of the vowel (1800 cps). The first-formant transition begins at 120 cps and rises to the steady state of the first formant at 480 cps.

The transitions of actual speech are not, of course, so angular as those shown in Fig. 2. We have found, however, that drawing the transitions as we have in that figure does not adversely affect the synthetic speech, and it does, for obvious reasons, enable us to control transition durations more precisely. Each formant consisted of a central strong harmonic and two flanking harmonics of lower intensity.

Presentation of stimuli.—As was pointed out in the introduction to Exp. I, we had thought it desirable to experiment separately with the two-category distinction between stop and semivowel and the three-category choice among stop, semivowel, and vowel of changing color. To investigate the distinction between stop and semivowel we assembled two sets of test stimuli. One, which we will call Test B–W, included 12 patterns from the series illustrated in the top row of Fig. 2 (rising second-formant transitions). The transition durations of these 12 patterns ranged from 10 to 120 msec. in steps of 10 msec., and had seemed on the basis of exploratory work to cover quite adequately the range from *bɛ* through *wɛ*. The other set of stimuli, which we will call Test G–J, included the 12 corresponding patterns from the bottom row of Fig. 2 (falling second-formant transitions). These had appeared in exploratory work to produce sounds that ranged from *gɛ* through *jɛ*.

For the three-category choice among *bɛ*, *wɛ*, and *uɛ*, we used all the patterns of the series illustrated in the top row to make up a set of stimuli that we will refer to as Test B–W–U. The series in the bottom row was used in connection with the distinctions among *gɛ*, *jɛ*, and *iɛ*, and will be referred to as Test G–J–I. For each of these two tests there was a total of 30 stimuli with transition durations that ranged from 10 to 300 msec. in steps of 10 msec.

The spectrographic patterns in each of the four tests were converted into sound and recorded on magnetic tape. By cutting and splicing the magnetic tape we assembled two random orders of stimulus presentation for each test. The signals were arranged on the magnetic tape in such a way that each stimulus would be presented and then repeated after an interval of 1 sec. There was an interval of 6 sec. between successive pairs of stimuli (i.e., a sound and its repetition). This interval provided time for *S* to make and record his judgment. The *S*s heard and identified each stimulus only once in Tests B–W and G–J. Half the *S*s heard the stimuli in one random order, and half in the other. The *S*s who identified the stimuli of Tests B–W–U and G–J–I made two judgments

of each stimulus, each of these tests having been given to the *Ss* once in each random order.

The *Ss* were told that each stimulus would be a synthetically produced syllable consisting of an initial speech sound followed in all cases by the vowel *ɛ*. They were asked to identify only the initial sound, and to limit their responses to the choices offered by *E*. For tests B–W, G–J, B–W–U, and G–J–I these choices were *b* or *w*, *g* or *j*, *b*, *w*, or *u*, and *g*, *j*, or *i*, respectively. Examples of these sounds, in syllabic contexts with the vowel *ɛ*, were given. The *Ss* were urged to make an identification of every stimulus, even though their judgments might in some cases be guesses. Before *Ss* made judgments in any test they were asked to listen to the first four stimuli of the series in order that they might become familiar with the nature of the sounds and the general method of presentation.

The four tests were variously interspersed among other tests involving synthetic speech (for example, groups of stimuli set up to study the fricative and nasal consonants), and sometimes two tests of the present experiment were given to the same group of *Ss* at the same session. The only restriction was that Test B–W was never paired with Test B–W–U, and, similarly, that Test G–J was never paired with Test G–J–I in the same session. An analysis of the results showed essentially no effect of the context in which a particular test was given (or of the random order used), so in reporting the results we have combined all the judgments for each test.

Subjects.—A total of 168 paid volunteers, all of them undergraduate students at the University of Connecticut, served as *Ss*. Of this group, 59 took Test B–W and 60 took Test G–J. The stimuli of Tests B–W–U and G–J–I were presented to groups of 41 and 49 listeners, respectively. Of the 41 listeners for Test B–W–U, 19 had previously heard and identified the stimuli of Test G–J; 22 of the 49 listeners for Test G–J–I had previously served in Test B–W. Prior to serving in this experiment, none of the *Ss* had had experience in identifying the synthetic speech sounds produced by the pattern playback.

Results

Figure 3 shows that our listeners were able to use the tempo of first-

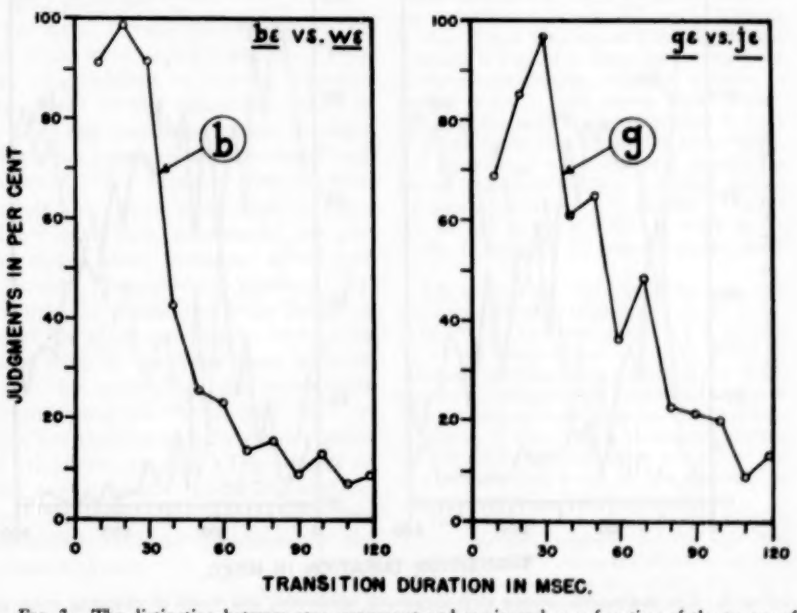

FIG. 3. The distinction between stop consonant and semivowel as a function of the tempo of transition. These curves show the percentage of stop consonant responses, and are based on the judgments of separate groups of 59 and 60 listeners for the *b–w* and *g–j* distinctions, respectively. Tempo is here expressed in terms of duration.

and second-formant transitions as a cue for distinguishing between stop consonant and semivowel. The change from *b* to *w* occurred when the duration of the transitions reached 40 msec., while *g* changed to *j* in the neighborhood of 50–60 msec. It is apparent from both curves that the very shortest duration of transition did not produce the best stop consonant.

Figure 4 shows the judgments we obtained when the range of transition tempos was increased and a third judgment category (*uɛ* or *iɛ*) was added. Apparently, our *S*s can reliably discriminate the three categories, stop, semivowel, and vowel of changing color on the basis of tempo of transition alone. It is also apparent, however, that the amount of agreement among our subjects was not so great in the three-category as in the two-category situations (cf. Fig. 3). The expansion of the stimulus range and the inclusion of the third judgment category obviously had the least effect on the responses to the stimuli at the short-duration end of the scale.

Experiment II

All the results of Exp. I, which indicated that transition tempo can distinguish stop consonant from semivowel from vowel of changing color, were obtained with the vowel *ɛ* following the initial transition. In Exp. II we have tried to find out how listeners respond to variations in transition tempo when vowels other than *ɛ* constitute the second part of the syllable. This experiment was

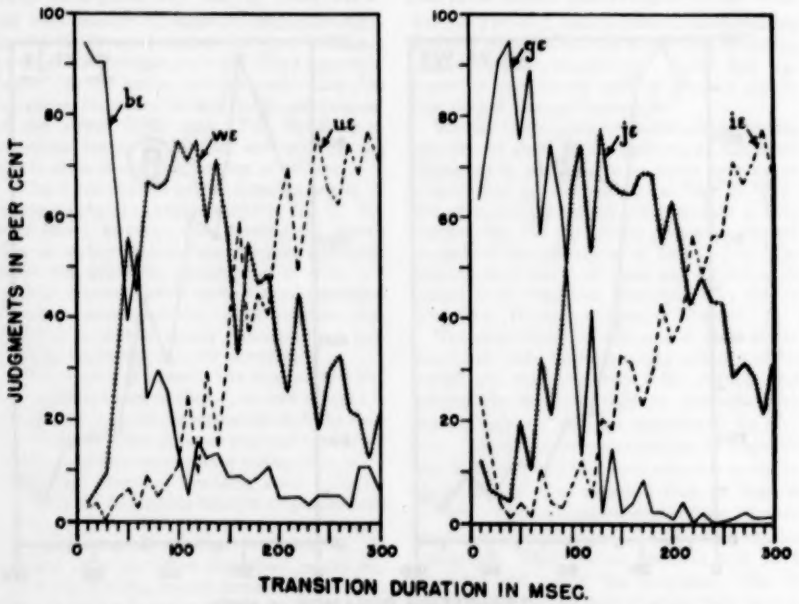

Fig. 4. The distinctions among stop consonant, semivowel, and vowel of changing color as functions of the tempo of transition. The curves representing the *bɛ–uɛ–wɛ* and *gɛ–jɛ–iɛ* responses are based on the judgments of separate groups of 41 and 49 listeners, respectively. Two judgments of all stimuli were obtained from each listener. As in Fig. 3, tempo is expressed in terms of duration.

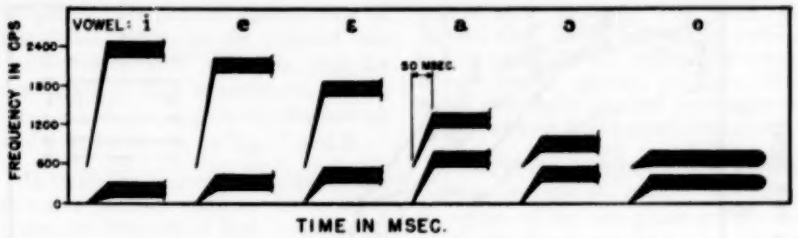

FIG. 5. Illustrations of the spectrographic patterns used to produce the stimuli of Exp. II. One only of the 15 transition durations, viz., 50 msec., is shown with each of the vowels. A complete stimulus pattern is shown at the extreme right.

concerned only with the distinction between the stop consonant *b* and the semivowel *w*. We have omitted the *g–j* distinction because to produce *g* with a variety of vowels requires a radical shift in the frequency level at which the second-formant transition begins, and introduces complications which are largely irrelevant to our present purposes.

In Exp. I rate and duration of transition were varied together. This was unavoidable, of course, because the extent of the transition—that is to say, the frequency range through which the formant moved—remained constant. To produce the various vowels that have been used in Exp. II we have had, necessarily, to put the steady-state formants at several different frequency levels. By starting the transitions from the same point for all these vowels, as can be done with *b*, we have been able to vary the extent of the transitions (from vowel to vowel) and thus to separate the rate and duration aspects of transition tempo. The results of this experiment should, therefore, help to determine which of these variables—rate or duration—is the controlling cue.

Method

Stimuli.—In Fig. 5 are samples of the spectrographic patterns that were used to produce the stimuli of this experiment. These patterns are the same as those of the B–W test of Exp. I except in three respects. The principal difference concerns the steady-state vowel. In Exp. I the first and second formants of the steady-state portion of the syllable were always set at frequency levels that would produce synthetic approximations to the vowel *ɛ*. (See Fig. 2). In Exp. II there are six different steady-state levels of the formants, these levels being appropriate to the vowels *i*, *e*, *ɛ*, *a*, *ɔ*, and *o*. The second difference is in the frequency at which the second-formant transitions begin—1080 cps in Exp. I and 600 cps in Exp. II. It was necessary in Exp. II to lower the starting point of the second-formant transition in order to produce *b* and *w* with vowels whose second-formant frequencies are as low as those of *ɔ* and *o*. For syllables that contain the other vowels, *i*, *e*, *ɛ*, and *a*, it is not necessary to start the second formant at so low a frequency, but, as we pointed out above, it is possible to do so.[8] We thought in this case that it would be desirable to start all the second-formant transi-

[8] To produce the very best *b*'s we must start the second-formant transition at levels higher than 600 cps, especially when the *b* is followed by a front vowel such as *i*, *e*, or *ɛ*. When we start this transition from a point as low as 600 cps, we not only produce a somewhat inferior *b*, but we tend in some cases to add at least a suggestion of *bw*. This is to say that starting the second-formant transition at or near the *b–w* locus is itself a cue for the semivowel *w*. For a discussion of "locus" see (**4**).

The addition of a small amount of *w* is counterbalanced, perhaps, by the fact that starting the first formant at 120 cps, as we do in all the patterns of this experiment, provides a cue for the stop consonant *b* as opposed to the semivowel *w*. To synthesize the best semivowel we must start the first formant at a somewhat higher frequency.

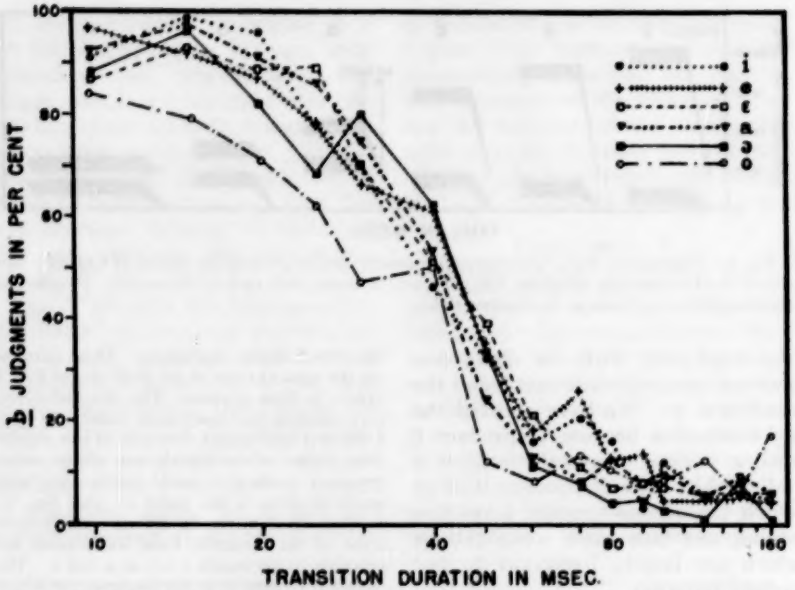

FIG. 6. The distinction between *b* and *w* with various vowels. These curves show the percentage of *b* judgments as a function of transition tempo when tempo is expressed as duration of transition. Each of 38 listeners judged all stimuli twice. Duration is scaled logarithmically to make these curves more directly comparable with those of Fig. 7 and 8.

tions at the same low frequency because in so doing we produced the greatest variation in extent of transition from vowel to vowel and thus obtained the greatest separation of rate and duration. The third difference between the patterns of Exp. I and those of Exp. II concerns the particular transition durations we chose to test. The results of Exp. I (see Fig. 3) had suggested that it would be most appropriate to sample the range 10 to 160 msec., using smaller steps at the short end and larger steps at the long end. Accordingly, we selected for use in Exp. II the following 15 values of transition duration for each vowel: 10, 15, 20, 25, 30, 40, 50, 60, 70, 80, 90, 100, 120, 140, and 160 msec.

Presentation of stimuli.—The 90 stimuli of Exp. II (15 transition durations times six vowels) were converted into sound on the pattern playback and recorded on magnetic tape. By cutting and splicing the magnetic tape we prepared two random orders of these sounds. The presentation of the stimuli was exactly as it had been in the B–W test of Exp. I, except that each *S* heard and judged the stimuli once in each random order. Thus, two judgments of each stimulus were obtained from each *S*.

Subjects.—There were 38 *S*s, all of whom were paid volunteer undergraduates at the University of Connecticut. None of the *S*s had previously served in Exp. I or had any other opportunity to hear the synthetic speech sounds produced by the pattern playback.

Results

We see in Fig. 6 that transition tempo is sufficient to distinguish the stop consonant *b* from the semivowel *w* with each of a wide variety of vowels. A comparison of the curves of Fig. 6 with those obtained for the vowel *ɛ* alone (see Fig. 3 in Exp. I) shows no very great difference.

When our listeners' judgments are plotted against transition duration, as in Fig. 6, the curves for the various vowels are very nearly superimposed. We may reasonably wonder what these curves will look like when the

judgments are plotted against rate of transition, since, as we pointed out earlier, rate and duration can be separated in this experiment. Such plots are shown, separately for second and first formants, in Fig. 7 and 8.

It will be remembered that our stimulus patterns were so drawn as to make the duration of first- and second-formant transitions always equal in any one pattern. Transition rates, on the other hand, are different for first and second formants inasmuch as the transition extents differ. In considering the responses as a function of rate, we must, therefore, deal with first and second formants separately.

We should note in regard to Fig. 8 that duration and rate of first-formant transition covary for the vowels *e–o* and, also, for *ɛ–ɔ*, since, as can be seen in the stimulus patterns of Fig. 5, the extent of first-formant transition is the same within each of these two pairs. It follows, then, that if the data for *e* and *o*, or for *ɛ* and *ɔ*, should yield similar curves when plotted against duration, they would necessarily produce similar curves against rate of first-formant transition.

Clearly the curves for the various vowels are not so nearly coincident for either of the rate plots as they are when the abscissa is laid out in terms of duration. This result would appear to support an assumption that duration of transition is the essential component of what we have been calling "tempo"—that is to say, that duration rather than rate is the controlling cue.

In assessing the relative importance of duration and rate we should take account of the fact that the rates of first- and second-formant transitions move in opposite directions in the vowel series from

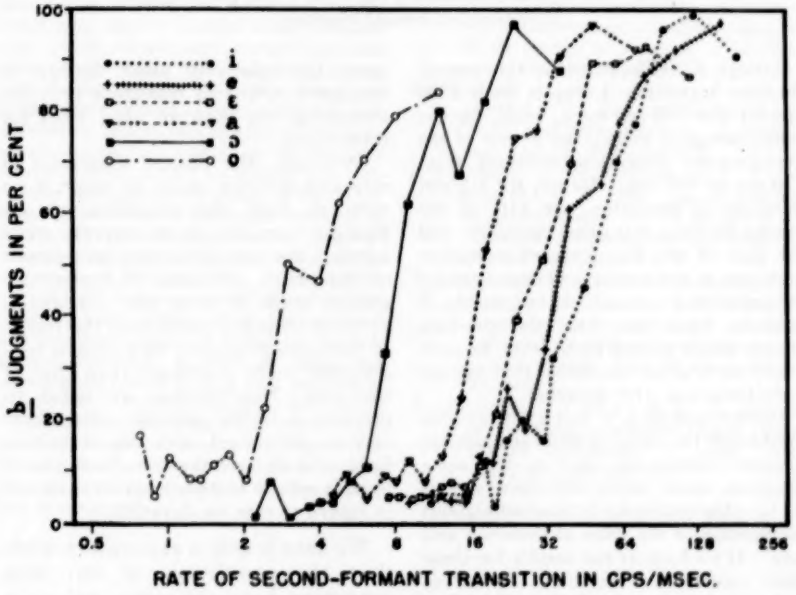

FIG. 7. The same data as shown in Fig 6, here plotted against rate of second-formant transition. Scaling the rates logarithmically serves to equate the distance on the abscissa occupied by the range of rates for each of the vowels.

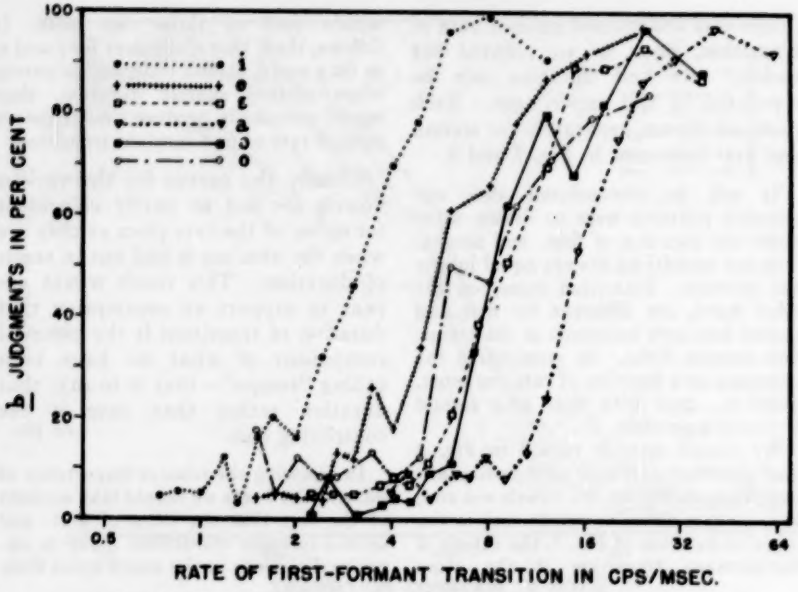

FIG. 8. The same data as shown in Fig. 6 and 7, here plotted against rate of first-formant transition.

i through *a*. (The extent of the second-formant transition decreases from 1800 cps for *i* to 720 cps for *a*, while, for the same change in vowel, the extent of the first-formant transition increases from 120 cps to 600 cps. Hence, for a given duration of transition, the rate of the second-formant transition decreases and the rate of the first-formant transition increases as the vowel is changed from *i* through *e* and *ɛ* to *a*.) It is possible, if unlikely, that the rate cues produce effects which cancel each other in such a way as to generate curves that appear to be invariant with duration.

For the vowels *a*, *ɔ*, and *o*, the extents (and hence the rates) of first- and second-formant transitions vary in the same direction, so we ought with these vowels to be able to make a less ambiguous comparison of the roles of duration and rate. If we look at the results for these three vowels, we see that the curves appear to be more closely bunched when plotted against duration than when plotted against rate, though in these cases the relatively small change in frequency extent of transition provides something less than an ideal basis for comparison.

Obviously, the clearest separation of rate and duration could be made if we were to hold the transition of one formant constant in all respects while varying the rate, duration, and extent of the other. A series of exploratory studies made it clear that for distinguishing stop and semivowel the tempo of the second-formant transition is considerably more important than that of the first. Nevertheless, we failed to produce a highly realistic series from stop to semivowel with one transition fixed, and we have therefore been unable by this means to obtain direct evidence in regard to rate vs. duration.

We have found in exploratory work that the conversion of the stop consonant *b* to the semivowel *w* is most effectively accomplished if, in addition to slowing the transitions,

we also make other changes in the spectrographic pattern. The accurate specification of these other cues and an evaluation of their relative contributions have yet to be made. At the present time we can only say that we have so far found no cue that promises to be more important than the time cue we have isolated in the present experiment. In any event, it is clear from the data reported here that the tempo or, more specifically, the duration of transition is a sufficient cue for distinguishing stop from semivowel from vowel of changing color. Thus, duration is to be added to the list of dimensions that are important for the discrimination and identification of individual speech sounds.

Summary

It had been shown in earlier research that the direction and extent of second-formant transitions enable listeners to distinguish speech sounds *within* each of the three classes, voiceless stops (*p–t–k*), voiced stops (*b–d–g*), and nasal consonants (*m–n–ŋ*). In distinguishing *among* these classes, listeners apparently depend on certain other cues, such as the presence of a nasal resonance or voicing, each of which serves as an acoustic marker for its class. In the first part of the present experiment it was found that, with all other things equal, the tempo of the transitions was sufficient to distinguish members of the class of voiced stop consonants from corresponding members of the classes semivowels and vowels of changing color. In the syllable consisting of the stop consonant *b* plus the vowel *ɛ*, the stop *b* was transformed into the semivowel *w* when the duration of first- and second-formant transitions exceeded 40 msec.; the corresponding change from *gɛ* to *jɛ* occurred at 50 or 60 msec. Further increases in the duration of the transitions caused *wɛ* and *jɛ* to become the vowels-of-changing-color *uɛ* and *iɛ*, respectively. The shift from semivowel to vowel of changing color was much less sharp than the change from stop to semivowel.

In the second part of the study it was found that transition tempo served equally well for distinguishing *b* from *w* with many vowels other than the one (*ɛ*) used in obtaining the results just described. By investigating the tempo cue with a variety of vowels, it was possible (as it had not been in the first part of the study) to assess the relative contributions of duration and rate of transition. The results suggest that duration is the significant aspect of the tempo cue.

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PREDICTION OF DECISIONS FROM A HIGHER ORDERED METRIC SCALE OF UTILITY[1]

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The purpose of this experiment was to compare the efficiency of two models for predicting a person's choices between alternative bets. One model bases all predictions on the person's previously determined higher ordered metric scale of utility for the commodity for which he gambles, asserting that he will make his decisions so as to maximize expected utility. The other bases all predictions on the objective or "face" value of the commodity, asserting that the person will make his decisions so as to maximize expected value. A secondary purpose of the study was to identify one source of those errors which do occur in predictions based on the higher ordered metric scale of utility.

The concept of *utility* (subjective value, or value to the person) is central in decision theory, a field which has been reviewed by Edwards (**5**). Recent research on the interval measurement of utility has been reported by Davidson, Siegel, and Suppes (**4**), and by Edwards (**6**). Coombs and Beardsley (**2**), Coombs and Komorita (**3**), Siegel (**8**), and Siegel and Beltz (**9**) have reported research concerned with ordered metric and higher ordered metric measurement of utility.

An ordered metric scale (**1, 8**) ranks the distances between an individual's preferences among certain commodities. It may be used with both discrete and continuous commodities. The method of higher ordered metric scaling devised by Siegel (**8**), which is based upon the work of Ramsey (**7**) and Von Neumann and Morgenstern (**10**), and which draws upon the developments of Davidson, Siegel, and Suppes (**4**), yields a preference scale containing the following information: "(*a*) it orders the entities involved, i.e., $A > B > C > D > E \ldots$; (*b*) it orders the distances between the entities, i.e., say $\overline{DE} > \overline{AB} > \overline{CD} \ldots$, where $\overline{AB}$ implies the difference in utility between A and B; and (*c*) it orders all possible combinations of contiguous distances between entities, i.e., say $\overline{AB} + \overline{BC} > \overline{BC} + \overline{CD} + \overline{DE}$, or $\overline{AB} > \overline{CD} + \overline{DE}$" (**8**).

In the present study, inmates in a federal prison were used as *S*s. Cigarettes, which are negotiable in prison society, were used as the commodity in the gambling situation, and each *S*'s utility of cigarettes was measured in a higher ordered metric scale. The information yielded by the study was used to test two hypotheses.

The first hypothesis concerned the relative predictive efficiency of the two models mentioned above:

1. *S*s *will make decisions so as to maximize expected utility rather than expected objective*

[1] The authors wish to thank the Federal Bureau of Prisons and the officials of the Northeastern States Federal Penitentiary for the cooperation which made this study possible. Financial support was provided in part by a fellowship granted to P. M. H. by the National Science Foundation, and in part by a grant to S. S. from the Council on Research of The Pennsylvania State University. Grateful acknowledgment is given to each.

[2] Now at Idaho State College.

value, and thus the predictions of decisions based on Ss' higher ordered metric scales of utility will be more accurate than those based on objective value.

The second hypothesis, stated below, was directed at identifying one source of error in prediction from a higher ordered metric scale. The differences in utility on the scale are used to predict *S*'s choices between alternative gambles. It is possible, however, that *S* might find a given set of two alternative gambles equally attractive or equally unattractive because they are based on practically equal distances, and would therefore be indifferent in his choice between them. In such a choice, it is reasoned that *S* might vacillate considerably before stating a decision. That is, the latency time between the offer of the choice and the statement of a decision would be long, reflecting *S*'s lack of a clear-cut preference. To test whether errors in prediction from a higher ordered metric scale are caused by the making of offers in which the two alternatives tend to be indiscriminable in expected utility, a second hypothesis was formulated:

2. *The latency times of those choices which are not predicted successfully by the principle of maximization of expected utility will be longer than the median latency time of all choices.*

Method

Subjects.—The *S*s were 30 inmates of the Northeastern States Federal Penitentiary, selected from 73 volunteers who responded to an advertisement placed in the prison newspaper. The advertisement requested volunteers for an experiment, and did not reveal the purpose of the experiment or that it involved gambling. It offered no inducement for participation by the volunteers except the opportunity to acquire a number of cigarettes. In the prison, cigarettes are a negotiable commodity whose status approaches that of a medium of exchange.

From the list of applicants, a list of 30 *S*s was compiled by selection on the basis of intelligence test scores (U. S. Public Health Service Intelligence Test, combined scores). It was deemed desirable to use as *S*s only those volunteers who were sufficiently intelligent to be able to understand the task required of them. The *S*s who served in the experiment had IQ scores which ranged from 104 to 131, with a median of 113.

Apparatus.—Sessions were held in a private room in the prison.

Two of the high-precision nonsense-syllable dice designed by Davidson, Siegel, and Suppes (**4**), each having two zero-association nonsense syllables symmetrically distributed over its six faces, were used to establish a gambling event having .50 subjective probability. For a detailed discussion of this device and its rationale, see (**4**) or (**8**). From a cup, a die was rolled by *S* against a backstop.

Paper was furnished to *S* to record the various offers and his choices in each case.

A stop watch was used to measure the latency of each decision. The timing was done by an inmate assistant who worked with *E*.

Procedure.—Two sessions, separated by a week, were held with each *S*. The same 30 offers, in the same order, were given in each session.

At the beginning of each session, *S* was handed seven packages of king-size cigarettes of a leading popular brand and was informed that these consituted his "starting stake." He was told that he might finish a session with well over a carton (10 packs) if he won, or that in the gambling he might lose part or all of his stake. He was told that whether he won or lost would depend on his skill in choosing the better alternative in each offer, and also on his luck in rolling the dice. The dice were then described to him. He was permitted to examine them and to make a few practice rolls, until he was convinced that they were fair.

The following portion of the standard instructions given each *S* shows how the data were then obtained: " . . . Now I'll give you a sample bet. This won't count. Write down 'win 30' and then below that write 'lose 10.' Use a plus to stand for 'win' and a minus to stand for 'lose.' Now across from that bet write 'win 5' and below that 'win 5.'"

The *E* then ascertained that *S* had written the offer in this form:

$$\begin{array}{cc} +30 & +5 \\ -10 & +5 \end{array}$$

"Now let's use this die, and I say 'QUJ is the winner.' If you choose the first bet (win 30 or lose 10) and if QUJ comes up on your roll, you win 30 cigarettes. If you had chosen the second bet, you'd have won 5 cigarettes. On the other hand, if you choose the first bet and then QUG, the loser, comes up, you lose 10 cigarettes. But

TABLE 1

OFFERS

Scale I		
1. +32 +14 −34 +14	6. +14 −3 −34 −3	11. +14 −3 −19 −3
2. +32 +14 −19 +14	7. +32 −3 −34 −19	12. −3 −19 −34 −19
3. +32 −19 −34 −19	8. +14 −19 −34 −19	13. +14 −3 −34 −19
4. +32 −3 −19 −3	9. +32 −3 −34 −3	14. +32 +14 −34 −19
5. +32 +14 −34 −3	10. +32 +14 −3 +14	15. +32 +14 −19 −3
Scale II		
1. +32 +17 −34 +17	6. +17 +1 −34 +1	11. +17 +1 −16 +1
2. +32 +17 −16 +17	7. +32 +1 −34 −16	12. +1 −16 −34 −16
3. +32 −16 −34 −16	8. +17 −16 −34 −16	13. +17 +1 −34 −16
4. +32 +1 −16 +1	9. +32 +1 −34 +1	14. +32 +17 −34 −16
5. +32 +17 −34 +1	10. +32 +17 +1 +17	15. +32 +17 −16 +1

if you'd chosen the second bet in that case, you'd have won 5 cigarettes anyway. Now which bet do you want?"

The *S* then chose a bet, and recorded his own choice by encircling it in pencil.

"Let's roll it off and see what would have happened."

The *S* then rolled the die. The *E* announced the outcome—how many cigarettes were won or lost on the roll.

The same procedure was then followed for three more sample bets, shown below, which illustrate the basic types of offers:

+ 10	− 25	+ 25	− 5	− 2	− 10
− 35	− 25	− 30	− 10	− 30	− 10

The remainder of the instructions consisted in showing *S* how to record and announce his choices, and in ascertaining that the entire procedure was understood by him.

The *E* then announced that he would make 30 offers in sequence, without stopping to roll the die to determine the outcome of each, and that all bets would then be "rolled off." That is, *S* was instructed to make all his choices between the alternative gambles before any die was rolled to determine any of the payoffs. It was clearly understood by *S* that he could not change his mind after having made a choice, and that after all 30 choices had been made the die would be rolled by him for each of them and the payoffs thus determined. The *S* was informed that the purpose of this procedure was to save time; actually the primary purpose was to guard against the effects of reinforcement, which could have changed *S*'s utility of cigarettes in the course of the measurement session.

The *E* concluded the instructions by announcing that there would be no more practice bets—that the forthcoming bets would "count." The 30 offers were then made, in the standard sequence shown in Table 1. In each case, *S*'s choice between the two alternatives was recorded by both *S* and *E*. Response latencies were timed and recorded by the assistant.

After all his choices had been made, *S* rolled the dice 30 times, and either received or paid out the required number of cigarettes after each roll. The two nonsense-syllable dice were alternated on successive rolls, and the winner was changed in roughly random manner. Thus the sequence of dice and winners followed the form shown in Table 2. The purpose of frequently switching from one die to the other was to guard against the common tendency to consider a particular outcome less likely on the forthcoming roll if it has just occurred on previous rolls. The purpose of switching winners was to reassure *S* that no attempt was being made to trick him with biased dice. To accomplish this, the winner was switched more than randomly often. Outcomes of the die rolls were recorded and totaled by both *E* and the assistant.

During *S*'s second session, the same procedure was employed as during the first session, except that the initial instructions were omitted. Questions relating to the procedure were answered by *E*.

Ordered metric scaling.—The method of

TABLE 2

SAMPLE SEQUENCE OF DICE AND WINNERS

Offer	Die	Winner
1	QUJ–QUG	QUJ
2	XEG–WUH	XEG
3	QUJ–QUG	QUG
4	XEG–WUH	WUH
5	QUJ–QUG	QUJ
6	XEG–WUH	XEG
.	.	.
.	.	.

TABLE 3

SAMPLE PROTOCOL: SCALE II

1.	+32 +17 −34 +17	AE < BB $\overline{AB} < \overline{BE}$	9.	+32 +1 −34 +1	AE < CC $\overline{AC} > \overline{CE}$
2.	+32 +17 −16 +17	AD > BB $\overline{AB} > \overline{BD}$	10.	+32 +17 +1 +17	AC > BB $\overline{AB} > \overline{BC}$
3.	+32 −16 −34 −16	AE > DD $\overline{AD} > \overline{DE}$	11.	+17 +1 −16 +1	BD > CC $\overline{BC} > \overline{CD}$
4.	+32 +1 −16 +1	AD > CC $\overline{AC} > \overline{CD}$	12.	+1 −16 −34 −16	CE < DD $\overline{CD} < \overline{DE}$
5.	+32 +17 −34 +1	AE > BC $\overline{AB} > \overline{CE}$	13.	+17 +1 −34 −16	BE > CD $\overline{BC} > \overline{DE}$
6.	+17 +1 −34 +1	BE < CC $\overline{BC} < \overline{CE}$	14.	+32 +17 −34 −16	AE > BD $\overline{AB} > \overline{DE}$
7.	+32 +1 −34 −16	AE > CD $\overline{AC} > \overline{DE}$	15.	+32 +17 −16 +1	AD > BC $\overline{AB} > \overline{CD}$
8.	+17 −16 −34 −16	BE > DD $\overline{BD} > \overline{DE}$			

scaling used here is a direct application of that developed by Siegel (**8**), and the reader is referred to that source for a full exposition.

The first 15 offers showa in Table 1 pertain to Scale I. The second 15 pertain to Scale II. In each session, thus, two higher ordered metric scales of utility of cigarettes were obtained from *S*. The second session constituted a replication of the first.

In Scale I, A = + 32, B = + 14, C = − 3, D = − 19, and E = − 34. Thus the objective scale of value (the scale representing "face value") is $\overline{AB} > \overline{BC} > \overline{CD} > \overline{DE}$ (read: the distance in objective value between A and B is greater than the distance between B and C, which is greater than the distance between C and D, . . .).

In Scale II, A = + 32, B = + 17, C = + 1, D = − 16, and E = − 34. Thus the objective scale of value is $\overline{DE} > \overline{CD} > \overline{BC} > \overline{AB}$.

When the offers are standard for all *S*s, as was the case in this study, from five to nine specific choices must be examined for the information necessary for constructing a higher ordered metric scale of utility. The remaining decisions yield "surplus" information which may be predicted from the other decisions, i.e., from the scale. Thus a minimum of six checks (predictions of decisions), and usually more, may be made for each scale. In the present study, a maximin method for deriving ordered metric and higher ordered metric scales was used. A maximin method is one which maximizes the minimum gain of information: the maximum amount of information is obtained from a minimum number of observations. The method used here maximizes the amount of information and therefore the number of predictions gained from observing a minimum number of *S*'s choices. This standard method dictates the *order* in which the various choices should be examined in the derivation of the scale. This order depends on the direction of each choice. In the maximin method, a chart is used which indicates the order of examination for all possible scales.[3]

[3] This method will be reported in a forthcoming paper by Siegel. In the meantime, copies of the table used for deriving scales by the maximin method are available from him.

For illustrative purposes, we have chosen one *S*'s scale for presentation. The protocol is shown in Table 3. For each of the 15 offers, the alternative chosen by this *S* is set in boldface. In the case of Offer 1, for example, *S* chose to win 17 cigarettes for sure, rather than to gamble on a 50–50 chance of either winning 32 or losing 34. Since $+17 = B$, $+32 = A$, and $-34 = E$, his choice is recorded as $AE < BB$. This is shown in the second column of Table 3.

Ordered metric scaling of utility is based on the fundamental proposition that if $AE < BB$, then $\overline{AB} < \overline{BE}$. That is, if *S* chooses the BB bet rather than the AE bet, then for him the difference in utility between B and E is greater than the difference in utility between A and B. For the rationale and proof of this, see Siegel (8). The third column in Table 3 translates each choice into information about distances on *S*'s utility scale.

All of the information in the third column of Table 3 is not necessary in order to construct a higher ordered metric scale. Only certain choices need be examined. The standardized method based on the maximin principle directs the search. According to that method, Offer 10 is examined first. It is observed that *S* chose the first alternative, indicating that for him $\overline{AB} > \overline{BC}$. The direction of this choice dictates that one turn to Offer 11. The direction of that choice leads one to examine Offer 13, after which the maximin principle directs attention to Offer 12. On the basis of *S*'s decisions in those four offers, one can construct his ordered metric scale of utility for the five amounts of cigarettes. The scale is: $\overline{AB} > \overline{BC} > \overline{DE} > \overline{CD}$. This may be contrasted to the objective scale for the values involved, which is $\overline{DE} > \overline{CD} > \overline{BC} > \overline{AB}$. The difference between *S*'s utility scale and the objective value scale demonstrates that for this *S* utility is not linear in cigarettes. The two scales also necessarily lead to different predictions.

The next step is to obtain the higher ordered metric scale. Following the maximin chart, one examines Offers 1, 2, and 6, in turn. From the seven decisions observed, *S*'s higher ordered metric scale of utility is constructed. It is: $\overline{AE} > \overline{AD} > \overline{AC} > \overline{BE} > \overline{AB} > \overline{BD} > \overline{CE} > \overline{BC} > \overline{DE} > \overline{CD}$. This scale is shown graphically in Fig. 1.

Only 7 of *S*'s 15 choices have been observed. The other 8 choices—the "surplus" choices—may be used to determine the correctness of the predictive model. In each case, there are two alternative models for prediction: (*a*) the expected objective value model, and (*b*) the expected higher ordered metric utility model.

Consider Offer 5. This offer concerns AE and BC. In terms of objective or "face" value, $AE < BC$. That is to say, the objective value model would predict the choice of the second alternative, because the expected value of the first alternative is -1 and for the second alternative it is $+9$. From observation of other choices, however, it has already been determined that in *S*'s higher ordered metric scale of utility, $\overline{AB} > \overline{CE}$. From the expected utility model, therefore, it is predicted that the first bet will be chosen, because if $\overline{AB} > \overline{CE}$, then $AE > BC$. This second prediction is the correct one, as Table 3 shows.

There were eight choices with regard to which predictions could be made. The reader will observe that for this *S* *all* of these choices were correctly predicted by the expected utility model. Only *half* of them were correctly predicted by the expected value model: these were Choices 3, 4, 7, and 8, which were correctly predicted from both models. The *S*'s choices on Offers 5, 9, 14, and 15 were *incorrectly* predicted by the objective value model but correctly predicted by the utility model.

Results

Of the 30 *S*s, 29 completed the experiment. The data from these 29 were analyzed to test the two hypotheses.

Hypothesis 1.—For each *S*, four higher ordered metric utility functions were derived, two for each session, according to the procedure discussed above. For each function there were 15 relevant decisions, including those central ones from which the function was derived and those "surplus" ones about which predictions could be made on the basis of the function.

The *S*'s choices which had not been used in deriving the function were checked for predictability under both models: a set of choices was checked for predictability from the higher

A B C D E

Fig. 1. Example of Higher Ordered Metric Scale II:
$\overline{AE} > \overline{AD} > \overline{AC} > \overline{BE} > \overline{AB} > \overline{BD} > \overline{CE} > \overline{BC} > \overline{DE} > \overline{CD}$

ordered metric utility function, and the number of errors recorded, and then the same set of choices was checked for predictability from the objective value function and the number of errors recorded.

For a given *S*, the total number of errors of prediction from the expected utility model was determined, as was the total number of errors of prediction from the expected value model. The difference between these two totals was the difference score, *d*, used in this statistical analysis. The magnitude of this *d* represents the comparative accuracy of the two models: the larger the *d*, the greater the relative predictive efficiency of one model over the other.

For the purposes of the nonparametric analysis employed, the *d*s were then ranked. Six *S*s had *d*s of 0; the remaining *d*s were ranked from 1 to 23. Fifteen of the *d*s were in the predicted direction, and the sum of their ranks was 206. Eight *d*s were in the opposite direction, and their sum of ranks was 70. Under the null hypothesis, these two sums of ranks would be expected to be about equal. The Wilcoxon matched-pairs signed-ranks test (**11**) demonstrated that the observed sums of ranks differed significantly from what would be expected under the null hypothesis: the data revealed that the expected utility model yielded significantly fewer errors of prediction than the expected value model ($P < .025$). This confirms Hypothesis 1.

Hypothesis 2.—For a given session, the latencies of the choices which were erroneously predicted by the expected utility model were compared with the over-all median latency for the 30 choices made by *S*. The number of erroneously predicted choices with less than median latency was subtracted from the number with greater than median latency. The indices thus obtained for *S*'s two sessions were summed algebraically to yield a single index for each *S*.

Four *S*s had *d*s of 0; the remaining *d*s were ranked from 1 to 25. Nineteen of the *d*s were in the predicted direction, and the sum of their ranks was 298. Six were in the opposite direction, and the sum of their ranks was 53. The Wilcoxon test demonstrated that the latencies of the erroneously predicted decisions were significantly greater than the median latency ($P < .001$). This confirms Hypothesis 2.

Discussion

The fact that decisions are more accurately predicted from *S*s' higher ordered metric utility functions than from the objective value function is direct evidence that for these *S*s utility is not linear in cigarettes (the social equivalent of money in this case). This finding is contradictory to that of Edwards (**6**) and supports the findings of Siegel (**8**) and Davidson, Siegel, and Suppes (**4**). The latter found that their technique for measuring utility in an interval scale yielded predictions which were 100% accurate for 15 of their 19 *S*s, all but one of whose utility functions deviated from linearity in money. Edwards' attempt at interval measurement of utility, with 5 *S*s, yielded predictions which were correct only 74% of the time, and which were exceeded (nonsignificantly) in efficiency by predictions based on objective value.

Although the most central reason for these differences in findings undoubtedly lies in differences in techniques of measurement, another reason seems to be that Edwards' selection procedure (inviting *S*s to participate in a gambling experiment), under which it was necessary to issue 23 invitations in order to obtain 5 *S*s, yielded a selected group of *S*s who were unusually willing to take risks and who might well be unusually experienced in gambling. In such persons, utility of money may well be more

linear than it is in most, and therefore the possibility that prediction from expected utility will improve over prediction from expected value is minimized. Moreover, Edwards' study extended over several months (thus requiring the assumption that utility remains stable over extended periods of time), and the utility curves he derived and used for prediction were based on averages. Inasmuch as he did not control reinforcement, it is possible that his finding utility curves which were close to linearity in money resulted from an artifact in his measurement. In the study reported here, the utility function was derived in about 15 min., at a single session, and reinforcement was adequately controlled during the measurement period. Insofar as Edwards may have succeeded in measuring utility, his results suggest the importance of certain parameters which enter into determining the nature of a person's utility of money. They cannot be taken as evidence which infirms the hypothesis of maximization of expected utility.

Summary

This study compares the efficiency of two models for predicting decisions in an uncertain outcome situation: the model based on the higher ordered metric measurement of utility, and the objective value model.

In the experiment, Federal prisoners served as *Ss* and cigarettes were the commodity. Two hypotheses were tested:

1. Subjects will make decisions so as to maximize expected utility rather than expected objective value, and thus the predictions of decisions based on *Ss'* higher ordered metric scales of utility will be more accurate than those based on objective value.

2. The latency times of those choices which are not predicted successfully by the principle of maximization of expected utility will be longer than the median latency time of all choices.

By a nonparametric test, Hypothesis 1 was confirmed at the $P < .025$ level, and Hypothesis 2 was confirmed at the $P < .001$ level. The first result not only favors the utility model but also demonstrates that utility is not linear in cigarettes, the negotiable commodity used, while the second suggests that errors of prediction from a higher ordered metric scale occur when one or more of the decisions on which the scale is based were made between two alternatives which were indiscriminable in expected utility. These findings support those of Davidson, Siegel, and Suppes (**4**) and of Siegel (**8**), and oppose those of Edwards (**6**). Although this discrepancy is doubtless primarily attributable to differences in measurement procedures and criteria, differences in procedures for selecting *Ss* may also contribute to it.

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